

THE  
MEDICAL NEWS AND ABSTRACT.

VOL. XXXIX. No. 11.

NOVEMBER, 1881.

WHOLE No. 467.

CLINICS.

Clinical Lectures.

ON INJURIES OF THE SPINE.

A CLINICAL LECTURE.

By WILLIAM HUNT, M.D.

SENIOR SURGEON TO THE PENNSYLVANIA HOSPITAL.

GENTLEMEN: The years circling about 1850 were troublesome times in Philadelphia. Political differences, race prejudices, an inefficient police, and, more than all, a volunteer fire department were causes of disorder and riot which at times raged rampant. The typical firemen of that day represented a race which is now extinct. Jakey, Mose, and Sykesey have disappeared. The only impress they have left is one upon the language of political slang. To "run with the machine," in those days meant what it said; now it means absolute fealty to party and obedience to the "boss."

What characters they were!

The rough of to-day only replaces them in his roughness. In no other respect does he resemble them. One of the most noble motives that can incite men to great deeds was the cause of their existence, that of saving life and property from destruction by fire. But the same enthusiasm that led them to do this, with a bravery and a risk to life and limb that were both reckless and astonishing, also excited such rivalry among the different organizations, that bitterness, enmity, and hatred, leading to frequent riots and bloodshed, were the result.

Jakey was a picture, with a high cocked hat surmounting his soaplocks, generally close shaved, except before parade days, when the big fellows let their beards grow, so as to appear as ferocious pioneers with their fire axes, prognathous, a defiant leer on his lip, between his teeth a segar held so tightly that its free end took an upward pitch, neck long, sinewy and open, a red cravat loosely tied with a large bow under a low-down collar, except when there was a red shirt, lithe and long of limb, arms akimbo and swinging free from the shoulder, a rough coat, long boots, with trowsers turned up at the bottom, here we have Jakey, brave to a fault and ready at any time for a fire or a fight.

Now what, I hear you say, has all this to do with a clinical lecture? It has this to do with it, that these same brave, rough and ready fellows,

always had a respectable quota of their number, whether from fire or battle, in the wards of this hospital. After a riot they fairly filled it. Let me give you an idea of the times we went through. I lived here day and night, in the latter part of 1849 and through the years 1850 and 1851. This was the only accident hospital at that time of any account in the city, and as a matter of course, nearly all of the wounded were brought to it. The firemen were generally good and grateful patients. Many a Moya, Fairmount, Goodwill, or Perseverance Hose fellow would have willingly risked his life for me. I nearly laid down my life for them. Two young resident doctors succumbed and died, greatly owing to their over-work and exposure in the house. I had an attack of typhoid fever, which became historic, and was a theme for the medical lecturers, in their clinics, for many years. I had, according to the opinions of most distinguished attendants (Drs. G. B. Wood, W. W. Gerhard, and the elder William Pepper), every symptom of having perforation of the bowels. The accident was made out completely, but I treated science so disrespectfully as to get well, and so gave no opportunity to confirm the diagnosis.

After one riot (which, by the way, I believe was more a riot of the mob against the firemen than the firemen with each other), there were admitted no less than ten seriously wounded men. The slightly wounded were dressed and sent home. Gunshot wounds of the skull, trunk, and extremities were among them. Three alone of this lot died. So vivid is the impression of these times, that I remember the cases perfectly, their respective beds in the wards, and even the names of some of them. There was McShane, with his brains oozing from a depressed gunshot fracture of the skull; Westerhood, with his leg shattered by a bullet; Shearer, with his bad wound of the thigh, and the genial Jim Beesley of savory memory.

It is no exaggeration to say that in this hospital we saw more cases of gunshot wounds, through those disturbed periods, than were seen by most medical officers of the army and navy in the times before the war. But sad to say, it was not always the participant in the battles who were struck. Women and children on the outskirts of the crowds were sometimes the victims. In this way little Johnny Farley, aged eleven, was fatally wounded. He was brought into my ward, and I at once saw that he had a compound comminuted fracture of the head of the left humerus, which would probably require operative interference. He was in profound shock, and before sending a note calling the visiting surgeon on duty, I resorted to the usual means to bring about reaction.

This was the case that thus early in my surgical life strongly impressed me with the phenomena that occur in serious injuries to the spinal column and cord or to either of them. In those phenomena, I have ever since taken a deep, though melancholy scientific interest, melancholy, because of the utter hopelessness of recovery for most of the victims; scientific, because these and brain cases are those which give us more knowledge of the physiology of the nervous system than anything else.

They are cruel, accident vivisections, vivisections performed on the highest of God's creatures. I dislike vivisection, although I have done something at it in my day; but I have frequently thought when standing helpless at the bedside of one of these terrible cases of spinal injury, that if a dog could be wounded and give me any light for the relief of the man, it ought to be done. I think of him who said "ye are of more value than many sparrows," and I say ye are of more value than a few dogs.

Now note what happened. After Johnny had warmed up a little in bed, I turned down the covers to apply artificial heat to his feet. He complained of pains in his legs, and I noticed something that then surprised me. Gradually his penis began to swell, and in a few moments complete priapism was established. I then felt his feet, and tickled them on the soles; there was response and complaint, but soon no notice was taken of what I was doing. I brought my hands up the limbs and found that both sensation and the power of voluntary motion were gradually going; soon they were entirely gone. The pains disappeared; nothing was complained of or felt on either side below a line a little above the umbilicus. The case was clear. The ball had not only broken the humerus, but it had passed along the posterior aspect of the thorax, and had wounded the spinal column and cord, and I caught a view of the phenomena that were thus produced, *right in the act*, as clearly as if I had performed a vivisection in the same region. In this case the cord, as was shown *post mortem*, was not entirely compressed or lacerated, otherwise complete paralysis would have come on at once. Its gradual approach was explained by partial wounding and pressure, which latter was made complete through effusion of blood, as reaction came on, and the force of the heart was restored.

The fracture was of one of the dorsal vertebra, at or below the third, for there were no nerve symptoms involving the upper extremities. The brachial plexus and the intercosto-humeral nerves were not affected. It was a matter of indifference where the ball was. It had either passed on, or it was lodged in the column where in fact it was afterwards found. Johnny lived seventeen days. His may be taken as the type of an acute case of severe wound of the spine and cord. He passed through all of the phenomena incident to such cases, and had one (which I shall discuss fully further on) to an extreme degree. The priapism continued unabated for fourteen days. What else? There was complete paralysis both of sensation and motion on both sides downwards, from a line just above the umbilicus. The functions of organic life went on. The patient breathed, ate, and slept, and his food was digested, voluntary power over the bladder was lost, and the urine had to be drawn off with the catheter; the urine itself became ropy with mucus, ammoniacal and fetid. The bowels, at first torpid, relaxed, and there were involuntary stools. The pulse was always rapid. We did not take definite temperature in those days, but there were alternations of heat and cold. I was stricken with the sickness to which I have alluded, a few days before the death of the patient. My friend Doctor, now Professor, Penrose took charge of the case. The *post-mortem* examination completely confirmed the diagnosis. The line of injury was through the shoulder-joint into the chest and along its posterior face. The 4th or 5th dorsal vertebra was broken in fragments, the cord was lacerated, and the ball was found projecting into the spinal canal, where it was held and supported by spiculae of bone. I looked over the University Museum yesterday for the specimen, where I thought it was; it was in the possession of Dr. Edward Peace, the chief surgeon for many years. I fear it has been lost.

I will now take up a case at the other end of my experience to this time, and then sum up what I have seen in a general way between the two periods. It also is typical, in that it illustrates a class which unfortunately are too common in accident hospitals. The victims truly "languish, and, languishing, they die."

Peter Green, adult, was admitted August 26, 1880. He died July 19, 1881—that is, 327 days after the injuries which killed him were received. He was in a stooping position, when a partition wall fell upon and crushed him.

The last dorsal and first lumbar vertebra were fractured, and a loose piece of one of them could be felt. There was immediate paralysis from the hips downward. In this case there was no priapism. The urine at first had to be drawn off with a catheter. The bowels were sluggish also, but soon became uncontrollable. Tympanites was a prominent feature. There was at times burning pain in the rectum. There was rapid emaciation, particularly noticeable in the limbs. At times there were attacks of local peritonitis. The pulse range for 42 days was from 108 to 72; temperature during the same period,  $101\frac{1}{2}^{\circ}$  to  $98\frac{1}{2}^{\circ}$ ; and respiration averaged 22.43. After this, as the case became chronic, and was almost one thing from day to day, the records were not regularly kept. Bed-sores formed, the urine dribbled, the feces oozed out. But the brain was perfect, the heart was sound, as also were the lungs; the stomach performed its digestive duties.

If no fortunate accident intervenes, as hemorrhage, for example, there is nothing for such patients but to be worn out. Food and care prolong their lives to an indefinite extent. It is nothing but a question of endurance. Barring accident or intercurrent disease, death takes place at last through exhaustion, and sometimes suddenly, from heart embolism, a stoppage of the circulation occurring at once, and thus mercifully ending a life which might still further be prolonged. Peter died from exhaustion. There was no autopsy granted, nor was it necessary. The case was too well known to require it.

Now during the time between these two cases, I do not know how many of the kind I have seen and have had under my care.

I have had them injured in every region of the column, from the atlas to the sacrum, cervical, dorsal, and lumbar, and in various parts of these regions. Understand, they do not all die, but the slightly injured, and even seriously injured, provided the cord is not too much involved, may get entirely well, or make partial recoveries. Sometimes the paralysis remains through a full length of days, and the patient is otherwise in reasonably good health. Again, the paralysis may be partial, or one side may recover to a much greater extent than the other. It is much easier to understand these cases in an anatomical, physiological, and pathological sense, than it is to cure them. Those who get entirely well are, I think, mostly cases of shock to the cord, through injury to the column, without lesion of its contents. In a letter I have written about President Garfield's case,<sup>1</sup> I have described such a case as this which came under my care last summer, and which fully proved that such shocks occur, for I had my fingers directly on the broken vertebral bones in the operation for removing the ball. Symptoms of shock to the column were present at first, but they rapidly disappeared. The President's case was also one of the kind, but the wound was in an inaccessible place. The spinal symptoms having gone so soon, is it any wonder that the true diagnosis was missed?

How easy such a diagnosis is when the symptoms remain and are more or less permanent, as in those I have described. As the letter is published

<sup>1</sup> See page 700.



in this number of the *Medical News* it is unnecessary to allude to it further. To give you an idea of the number of cases that are sometimes under notice, I remember on one occasion that Dr. Weir Mitchell asked me to let him know when I had any of them in my wards, as he wished to study certain points about them. I was able at that very time to show him five—three on one side, and two on the other, of the long ward alone. I have not given you nearly all the points, but only the striking ones. This is a clinical lecture, and the subject is one for a volume. Volumes have been written on it. I have not referred to them, but only give you a hasty sketch of my own experience. I will now relate another case, to show what inflammation of the membranes and softening of the cord do, where there is no injury to the bone, and thus you will understand how the bones, provided they are not displaced, are not at all necessary factors in producing the dreadful symptoms I have described, and also how, through the application of anatomy and physiology, we are sometimes able to locate the trouble precisely.

In the summer of 1859, a gentleman, 52 years of age, began to complain of pain posteriorly, over a limited space, at the root of the neck, which at first was slight, but it was continuous and annoying. There were also sensations of a peculiar kind down both extremities, more of uneasiness and want of confidence than of pain. The fingers became numb, and things did not feel right in them; this was more noted in the right hand than the left, but only owing to the fact that the use of the right was wanted more than the left, and consequently its defects were noted. At this time there was no tottering in gait. Travel was tried for a few weeks, but the patient returned somewhat worse than when he went away. The symptoms were more pronounced. The pain was greater, the gait somewhat uncertain, a pen could not be held rightly. There was no trembling nor spasmodic jerking. The functions of the body were properly performed; the intellect was clear; there was some mental depression and anxiety. Business, however, was attended to. The patient was president of a corporation. Counter-irritants and other remedies were of no avail to check the progress of the disease.

The pains became more severe and sometimes were very bad; they did not spread, but were distinctly localized, and kept to the same region. The difficulties about the extremities grew worse. Finally the patient stayed at home, and soon took to his bed. His sufferings increased, and then he lost the use of all his limbs—of the legs first, and then of the arms. There was no priapism. The diagnosis was, that there was, first, a local spinal meningitis, and a subsequent inflammation and softening of the cord. The pain became intense; as a measure of the suffering I shall never forget what he said when the actual cautery was applied to the back. Dr. Joseph Pancoast, who attended the case with me, did the operation. The patient would not take ether, and as the white hot iron was seething through his skin and flesh, clear down to the bone, he said, "Oh, that is better than the pain!"

But all efforts to check the disease were in vain. Paralysis of the animal life of all parts below the root of the neck came on. Voluntary motion and sensation here were gone. We knew the part of the cord affected was below the origin of the phrenic nerve, for the diaphragm acted perfectly. It was by this muscle and through physical laws that the patient breathed. We knew the lesion was just below this nerve, for the parts supplied by the brachial plexus were paralyzed.

The intercostal muscles did not act. The abdomen became enormously distended, by reason of flatus accumulating in the bowels, the abdominal muscles having no power to control or repress it. In this case I did what I have never done since, although I have tried it. I have seen it denied in a French journal that it can be done. Regularly every morning, with a large stomach tube, which I passed as much as eighteen inches up the bowel, I drew off the flatus and relieved the pressure, as an engineer would relieve the pressure of steam on his boiler. The abdominal walls fell flaccid, but incapable of resisting, they arose again from the same cause in the following twenty-four hours. The urine, which early in the case had to be drawn away, now dribbled, and a flexible catheter was kept in the bladder. Peripheral nutrition was interfered with; there were huge sloughs of the skin, particularly of the back, in spite of all that wealth could purchase, in the way of peculiarly constructed beds and cushions, and all that the best nurses could do. The fault was within, and not without. The patient was vegetating below the root of the neck; all there was of animal life in him was above it. The heart, supplied from its cardiac plexus, most of which comes from above the origin of the brachial nerves, continued to act; the stomach did its digestion; the sympathetic nerve, which was not involved, regulated the secretions and the supply of blood to it, which in its turn it furnished with new material. But day by day there was loss, and death took place in the beginning of the following year, six months having been required with all these terrible things to destroy life. Now what was the cause of all this? The post-mortem examination, made by Dr. James Darrach and myself, showed it to be an inflammation of the membranes of the cord and a softening and destruction of the cord itself, for a space of about two and a half inches. The lesion was below the phrenic nerve, and where the cord gives off its branches to form the brachial plexus. *Everything else was normal.*

I will now detail a case of severe spinal shock. On the 6th of May, 1881, a man aged 21 was brought to the Hospital, paralyzed in both lower extremities. He fell from a height (about twenty feet) and landed directly on his feet. It was at first thought there was fracture of the lumbar vertebra, but nothing of this kind could be found on examination. Both calcanea were found to be fractured. The escape of the spinal column from severer injury was thus explained by the feet receiving the greater force of the blow, as much of it must have been expended in breaking such well-protected and thick bones. Enough of the shock however was transmitted to the spinal cord to produce most serious results. The paralysis of the left leg was not so great as that of the right one. There was dribbling of urine from loss of control of the bladder and overdistension. The catheter had to be used for a few days. There was no priapism. The bowels were torpid, and there was tympanites. There was much pain in the legs. Sensation was not affected at any time to the same degree as motion, although it was impaired. Nutrition went on uninterruptedly. The bones of the feet united as rapidly and as firmly as though nothing else was the matter. There was no particular trouble with bedsores. To aid in preventing them the patient was at first put on a water-bed.

In a short time he became very tired of the unsteadiness of this, and asked for an ordinary bed. His request was granted without any bad result. Whilst his bones were mending, the spinal trouble was slowly and surely disappearing. Sensation improved quite rapidly. On the 31st of

the month the patient was able to lift the left leg. Through the following two months he gained power over the right one, and in the mean time perfect control was gained as to the other functions.

The injuries of the feet and cord together required the use of crutches after the patient left his bed. He soon learned to walk, and was discharged on the 31st of August.

Contrast this case with the one of gunshot fracture of the spine and that of myelitis or inflammation of the cord which I have given you, and there will be no difficulty in noticing the distinctions. Sometimes after such falls the feet take all of the force of the blow. I once saw a man who fell in the same way as the one I have just described, but from a much greater height. The bones of both legs were driven out at the ankle-joints, and the limbs had to be amputated. There were no spinal symptoms whatever in this case. Some cases of spinal shock are so severe as to go on to death through disorganization of part of the cord. Then the progress is like one of progressive myelitis.

The way in which the spinal cord is suspended in the canal without nearly filling it, as the brain fills the cavity of the cranium, is the reason that some surgeons deny the occurrence of spinal shock; what they mean is that some lesion takes place at the same time, for instance a local stasis of blood, or may be ruptures of small vessels. The same thing is more than likely true of many concussions of the brain, which all admit occur frequently. Concussions of the spine are of all grades from the unpleasant temporary jar that one gets when he is ignorant of or does not see a step in his way, and suddenly leaves a level; to the severe ones, such as that which has been described.

Railroad presidents and officials, and their lawyers, are the greatest skeptics about spinal concussion. The reason is, that it is the injury above all others that is simulated by dishonest claimants for damages after railroad accidents, so much so indeed that the name of "railroad spine" has absurdly been given to the cases. Spinal shock from other causes does not differ in its symptoms and progress from that which is produced on railroads. It is the degree of all of them that determines their gravity.

The possible results of a severe spinal shock can hardly be exaggerated. At the same time its symptoms and progress have been studied and imitated with a most astonishing minuteness. Judges, lawyers, and doctors have all been deceived, for after a supposed most pitiable victim of injury has recovered damages, he has also recovered health.

You should study these cases well, otherwise one of these doubting lawyers will twist you on the witness stand worse than the claimant's spine has been wrenched. I believe in spinal shock whatever its pathology may be, but I also know that it is a most difficult matter to distinguish the real from the spurious cases, especially where there is an insufficient or vague account of the accident which is supposed to have caused it.

I said I would discuss the symptom of priapism, which so often takes place after injuries to the spine, and which, if it does occur, is such a sure index of that injury. I have seen it follow, after accidents to all parts of the column, from the axis, of which I remember one particular case where it was present, down to the fifth lumbar vertebra. It is by no means a constant symptom. It may be but momentary, and, as we have seen, it may continue for days.

Also, as shown in the case I have last described, it is not apt to come on in disease of the membranes and cord, not the result of injury.

It may show itself rather late in such a case by extension outwards, but I have never met with it.

All these facts are puzzling, and I have never found a satisfactory explanation of them until now it dawns upon me. I suppose the same thing is in some book or lecture, but I have never seen it, and can not look for it now. I know this: that I have asked men most learned in nerve matters about it, and they have given me no satisfaction, and have acknowledged the puzzle. The fact is, we do not want the cerebro-spinal axis at all to explain it. Hilton, I think, goes to the brain for a reason for it, but we do not want the brain. We want the brain and axis to explain normal erection where there is sensation accompanying it, for in the form I am considering there is no sensation.

Now call to mind the sympathetic system of nerves. It is composed of a chain of ganglia and peculiar nerves lying along the spinal column, on the outside of the vertebral bodies, near the junctions of the ribs with the vertebra, and the intervertebral foramina, in the dorsal region; further forward on the bodies, in the lumbar region; and latterly with very long ganglia, in the cervical region.

Again, you all remember the rabbit that is brought out, in the lectures on inflammation, to show how the bloodvessels of the ear become engorged when the sympathetic nerve is divided in the neck. Whatever else this nerve may do, it is acknowledged to be the great vaso-motor regulator of the bloodvessel system; it is, as Draper says, the "fly-wheel" of the economy. The fibres of this nerve are abundantly distributed along the spermatic cord, which is mainly composed of the bloodvessels of the genital organs and the spermatic duct. In a normal erection the inhibitory power of the sympathetic is overcome by the fierce intensity of sexual emotion transmitted through the brain and cord. Local irritation through reflex also conquers the inhibition. This inhibitory power is also overcome when the trunk or ganglia of the sympathetic is abnormally irritated, lacerated, or divided. When this is the case, the blood rushes into and engorges the highly vascular tissue of the penis, and erection takes place, exactly as it rushes to the ear of the rabbit when the nerve is cut in the neck. If the ear was composed of what is called erectile tissue, which is highly vascular and elastic tissue, with fine muscular fibres, it would swell enormously from the same cause.

Thus it is not necessary to trouble one's self about a special nerve centre ruling over the function we are considering. Thus also this symptom in accidents to the column and cord is explained; also why some have it and others do not, why it is evanescent in some and in others almost constant, why it may appear in injuries occurring anywhere along the spine, why there is no sensation when it occurs as an accompaniment of laceration or pressure on the cord, for communication with the brain is cut off, and why also it is rare to find it in disease of the membranes or cord.

When the wounding missile or crushing force breaks the spine and involves the cord, and there is accompanying priapism, then the neighbouring ganglia or nerves of the sympathetic are bruised or wounded at the same time, from being caught in the line of the crush or wounding missile. When this symptom does not occur, the sympathetic has escaped. I have no doubt that when it occurs in hanging, the superior cervical ganglia of the nerve are caught in the squeeze. In most cases of disease the lesion is within the canal, and the sympathetic fibres or ganglia are not involved.

I shall say but little of the treatment of these cases. They are those

to which as much as any other class the popular solecism is applied, that "the doctor can do nothing for them," meaning that he cannot cure them. Do nothing for them? Why everything has to be done for them. The bad ones are themselves helpless.

How hospital nurses dislike to see them come into the wards! How hospital governors and managers deplore the expense of maintaining them! How residents and chiefs get tired of seeing them, day after day, slowly but surely losing ground! What a relief it is to make a morning visit and find the long occupied bed empty! There is respite for a while only, for soon another will come in. Individuals perish, the race survives, and so it is with accident and disease. We have their victims always with us. Where we cannot aid to bring about recovery, we can alleviate and prolong life, although, if we were the judges, we would often say it was not desirable to do so. The details, for instance, of what was done for Peter Green, during the 327 days he was in the hospital, would fill pages, and I have not time to give to them now.

---

#### ADDRESS ON PUBLIC MEDICINE.

*Delivered at the Opening of Section XIII. State Medicine, of the International Medical Congress.*

By JOHN SIMON, C.B., F.R.C.S., D.C.L., LL.D.

GENTLEMEN: In preventive, just as in curative, medicine, it occasionally happens that consequences more or less valuable result from some mere chance-hit of discovery; but, except so far as this may sometimes (and but very rarely) happen, disease can only be prevented by those who have knowledge of its causes—knowledge which does not deserve to be called knowledge, unless in proportion as it is conclusive and exact; and thoroughly to investigate the causes and their mode of operation is the quite indispensable first step towards any scientific study of prevention. Essentially, we know how to prevent by having first learnt exactly how to cause. Therefore it is that preventive medicine has had almost no development until within these later times. The germinal thought of it may be traced in even the first days of our profession. The spirit in regard of which Hippocrates has been aptly called the Father of Medicine—the scientific spirit of observation and experiment, as distinguished from the spirit of priestcraft, was one which his medical writings equally showed in their preventive as in their curative relations; and when he, some twenty-three centuries ago, expounded to his contemporaries that pathology is a branch of the science of nature—that causes of disease are to be found in physical accidents of air and earth and water, and in quantities and qualities of food, and in personal habits of life, he (not without risk of being denounced for impiety) virtually proclaimed for all time the first principle of preventive medicine, and indicated to his followers a new line of departure for those who would most largely benefit mankind. His followers, however, have had their work to do. True knowledge of morbid causes could only come by very slow degrees, and as part of the development with which the physical and biological sciences have, little by little, with the labour of ages, been building themselves up; and so no wonder that, despite the lapse of



time, even the most advanced of nations are hitherto but beginning to take true measure of the help which preventive medicine can render them.

Now what is the nature of that study of causes through which we may gradually arrive at counter-causing or prevention?

Addressing a skilled audience I shall utter what to them is the merest commonplace when I say that, in the physical and biological sciences, we acknowledge no other study of causes than that which consists in *experiment*. And the study of morbid causes is no exception to that rule; it is solely by means of experiment than we can hope so to learn the causes of disease as to become possessed of resources for preventing disease.

The experiments which give us our teaching with regard to the causes of disease are of two sorts—on the one hand we have the carefully prearranged and comparatively few experiments which are done by us in our pathological laboratories, and for the most part on other animals than man; on the other hand we have the experiments which accident does for us, and, above all, the incalculably large amount of crude experiment which is popularly done by man on man under our present ordinary conditions of social life, and which gives us its results for our interpretation.

When I say that experiments of those two sorts are the sources from which we learn to know the causes of disease, I, of course, do not mean that the mental process by which an experiment becomes instructive to us is the same in regard of the two sorts of experiment. On the contrary, the etiological problem (so long as it is a problem) is approached in the two cases from two opposite points of view; and the dynamical continuity of relation, which we call cause and effect, is traced, in the one case from the one pole, and in the other case from the other pole of the relation. In the one case, starting with knowledge of our own deliberately-prepared *cause*, our question is, What will be its effect? In the other case, starting from a certain *effect* presented to us, our question is, What has been its cause? But in the second case, just as in the first, when the question is answered, when the problem is solved, when the relation of cause and effect has been made clear, we recognize that the conjuring power which has brought us our new knowledge is the power of a performed experiment.

Let me illustrate my argument by showing you the two processes at work in identical provinces of subject-matter. What are the classical experiments to which we habitually refer when we think of guarding against the dangers of Asiatic cholera? On the one side there are the well-known *scientific* infection experiments of Professor Thiersch, and others following him, performed on a certain number of mice. On the other hand, there are the equally well-known *popular* experiments which, during our two cholera epidemics of 1848-9 and 1853-4 were performed on half a million of human beings, dwelling in the southern districts of London, by certain commercial companies, which supplied those districts with water. Both the professor and the companies gave us valuable experimental teaching as to the manner in which cholera is spread. I need not state at length the facts of those experiments, probably known to all here, but may rather justify my parallel by referring to an etiological question which will presently be discussed in our Section. It concerns the *causation of tubercle*—the most fatal by far of all the diseases to which the population of this country is subject. On that subject for the last sixteen years we have had a new era of knowledge. It was the great merit of a Frenchman, M. Villemin, that he, in 1865, first made us fully aware that tubercle is an infectious disease. He did this by certain laboratory experiments performed on other animals than man. He found that general and fatal tubercular infection of the animal was produced when he inoculated it subcutaneously with a little crude tubercular matter from the human

subject. That first laboratory investigation of the subject has been followed most extensively by others; and the further experiments, while entirely confirming M. Villemin's discovery, have shown that subcutaneous inoculation is not the only mode by which the tubercular infection can be propagated. Dr. Tappeiner and others have shown that the same effect is produced on the animal if tubercular matter (such as the sputa of phthisical patients) be diffused in spray in the air which the animal breathes; and Professor Gerlach, of Hanover, showed twelve years ago with regard to the bovine variety of tubercular disease (the *Persucht* of the Germans) that its infection can be freely introduced through the stomach if bits of tubercular organs be given in the food, or if the healthy animal be fed with milk from the animal which has tubercle. That the communicability of tubercle from animal to animal is also being tested to an immense extent by popular experiments on the human subject is what a moment's reflection will tell; and from that wide field of experiment I select one instance for illustration. I have every reason to believe that Professor Gerlach's experiments on the communicability of tubercle by means of milk are very extensively parodied by commercial experiments on the human subject. I learn, on what I believe to be the highest authority in this country, that tubercle (in different degrees) is a malady which abounds among our cows; and that so long as the cow continues to give milk, no particular scruple seems to prevent a distribution of that milk for popular use. To the persons who consume that milk an important question as to the causability of tubercle is put in an experimental form. Whether they will become infected with tubercle is a question which the individual consumers do not stand forward to answer for themselves, like the animals of the laboratory experiments; but Dr. Creighton's lately published book, entitled "*Bovine Tuberculosis in Man*," and a paper in which I am glad to say he brings under notice of our Section the very remarkable series of facts on which he grounds that startling title, seem to suggest a first instalment of answer in accordance with Professor Gerlach's experimental finding.

The two sorts of experiment—the scientific and the popular—differ, as I have noted, in this particular: that the popular experiment is almost always done on man; the scientific almost always on some other animal. It is true that many memorable cases are on record, where members of our profession have deliberately given up their own persons to be experimented on by themselves or others for the better settlement of some question as to a process of disease; have deliberately tried, for instance, whether, in this way or in that, they could infect themselves with the poison of plague or of cholera; and as regards one such case which is in my mind, I think it not unlikely that the illustrious life of John Hunter was shortened by the experiments which he did on himself with the ignoble poison of syphilis. There have been cases, too, where criminals have been allowed to purchase exemption from capital or other punishment at the cost of allowing some painful or dangerous experiment to be performed on themselves. And cases are not absolutely unknown where unconsenting human beings have been subjected to that sort of experiment. But waiving such exceptions, the rule is, as I have said, that scientific experiments relating to causes of disease are performed on some animal which common opinion estimates as of lower importance than man. Now, as between man and brute, I would not wish to draw any distinction which persons outside this room might find invidious; but, assuming for the moment that man and brute are of exactly equal value, I would submit that, when the life of either man or brute is to be made merely instrumental to the establishment of a scientific truth, the use of the life should be economical. Let me, in that point of view, invite you to compare, or rather to contrast with one another, those two sorts of experiment from which we have to

get our knowledge of the causes of disease. The commercial experiments which illustrated the dangerousness of sewage-polluted water-supplies cost many thousands of human lives; the scientific experiments which with infinitely more exactitude justified a presumption of dangerousness, cost the lives of a few dozen mice. So, again, with experiments as to the causation of tubercle; judging from the information which I quoted to you, I should suppose that the human beings whose milk-supply on any given day includes milk from tubercular cows might be counted, in this country, in tens of thousands; but the scientific experiments which justify us in declaring such milk-supply to be highly dangerous to those who receive it were conclusive when they amounted to half a dozen. So far, then, as regards the mere getting of experimental knowledge, we must not, with a view to economy of life, be referred to popular, rather than scientific, experiment. And, in the same point of view, it perhaps also deserves consideration that the popular experiments, though done on so large a scale, very often have in them sources of ambiguity which lessen their usefulness for teaching.

Let me now briefly refer to the fact that, during the last quarter of a century, all practical medicine (curative as well as preventive) has been undergoing a process of transfiguration under the influence of laboratory experiments on living things. The progress which has been made from conditions of vagueness to conditions of exactitude has, in many respects, been greater in these twenty-five years than in the twenty-five centuries which preceded them; and with this increase of insight, due almost entirely to scientific experiment, the practical resources of our art, for present and future good to the world, have had, or will have, commensurate increase. Especially in those parts of pathology which make the foundation of preventive medicine, scientific experiment in these years has been opening larger and larger vistas of hope; and more and more clearly, as year succeeds year, we see that the time in which we are is fuller of practical promise than any of the ages which have preceded it. Of course, I cannot illustrate this at length, but some little attempt at illustration I would fain make.

First let us glance at our map. When we generalize very broadly the various causes of death (so far as hitherto intelligible to us), we see them as under two great heads, respectively autopathic and exopathic. On the one hand there is the original and inherited condition under which to every man born there is normally assigned eventual old age and death, so that sooner or later he "runs down" like the wound-up watch with its ended chain; and, as morbidities under this type, there are those various original peculiarities of constitution which make certain individual tenures of life shorter than the average, and kill by way of premature old age of the entire body, or (more generally) by quasi-senile failure of particular organs. On the other hand, as a second great mass of death-causing influence, we see the various interferences which come from outside; acts of mechanical violence for instance, and all the many varieties of external morbid influence which can prevent the individual life from completing its normal course.

As regards cases of the first class—cases where the original conditions of life and development are such as to involve premature death (which in any such case will commonly show itself as a fault in particular lines of hereditary succession)—the problem for preventive medicine to solve is, by what cross-breeding or other treatment we may convert a short-lived and morbid into a long-lived and healthy stock; and this, at least as regards the human race, has, I regret to say, hardly yet become a practical question. But as regards cases of the second class, evidently the various extrinsic interferences which shorten life have to be avoided or resisted, each according to its kind; and here it is that the scientific experimenters of late years have been giving us almost daily increments of knowledge. Two early instances, vastly important in themselves, though of a comparatively

crude kind, I have already mentioned; and I now wish to glance at some illustrations of the immense scope and marvellous exactitude of the newer work.

The invaluable studies of M. Pasteur, beginning in the facts of fermentation and putrefaction, and thence extending to the facts of infectious disease in the animal body, where M. Chauveau's demonstration of the particulate nature of certain contagia came to assist them—they, I say, partly in themselves, and partly in respect of kindred labours which they have excited others to undertake, have introduced us to a new world of strange knowledge. We have learnt, as regards those diseases of the animal body which are due to various kinds of external cause, that probably all the most largely fatal of them (impossible yet to say how many) represent but one single kind of cause, and respectively depend on invasions of the animal body by some rapidly self-multiplying form of alien life. This doctrine, which scientific experiment initiated, has for the last dozen years been extending and confirming itself by further experiment. As soon as the doctrine began to seem probable, science saw that, should it prove true, it must have been the most important corollaries. If the cause of an infecting human disease is a self-multiplying germ from the outside world, the habits of that living enemy of ours can be studied in its outside relations. It becomes an object of common natural history; it has biological affinities and analogies. We can cultivate it in test-tubes in our laboratories, as the gardener would cultivate a rose or an apple, and we can see what agrees and what disagrees with its life. And then, as the next and immeasurably most important stage, where nothing but experiment on the living body will help us, we can try whether perhaps any of our modifications of its life have made it of weaker power in relation to the living bodies which it invades, or whether, through our more intimate knowledge of its vital affinities, we can artificially give to bodies which it would invade a partial or complete protection against it. Such, at first blush, were the obvious possibilities of research which the new doctrine of infectious disease suggested to the mind of the pathologist; and never since the profession of medicine has existed had a field of such promise been before it. The promise has not been belied. A host of diseases has been worked at in such lines as I just now indicated, and with many of them important progress has been made.

It would be impossible for me even to name a twentieth part of the investigations which have been more or less successful. As regards some which have most struck me, I pass with but a word—Dr. Klein's investigation of the pneumo-enteritis of swine; Professor Cohn's and Dr. Koch's and Dr. Buchner's respective contributions to the natural history of the anthrax bacillus; Dr. Böllinger's recognition of the microphytic origin of an important canceroid disease of horned cattle; with Dr. Johne's illustration of the inoculability of this disease; the research of Drs. Klebs and Tommasi-Crudeli into the intimate cause of malarial fever; and, not least, the demonstration (as it appears to be) which Dr. Grawitz has recently published, that some of the commonest and most innocent of our domestic microphytes can be changed by artificial culture into agents of deadly infectiveness. I pass these and others in order that I may more particularly speak of some which have already shown themselves practically useful; for in respect of some of them the time has already come when abstract scientific knowledge is passing into preventive and curative act.

First, and not in a spirit of national partiality, I will mention the application which M. Pasteur's doctrine has received at the hands of Mr. Lister, with regard to the antiseptic treatment of wounds, an application which, enforced and illustrated at every turn by Mr. Lister's own eminent skill as an experimentalist, has been confirmation as well as application of the parent doctrine, and the beneficent uses of which, in giving comparative safety to the most formidable surgical opera-

tions, and in immensely facilitating recovery from the most dangerous forms of local injury, are recognized—I think I may say by the grateful common consent of our profession in all countries—to be among the highest triumphs of preventive medicine.

Secondly, out of the experimental studies of anthrax—chiefly out of those of Dr. Sanderson and Mr. Duguid in this country, and those of Dr. Buchner in Germany, and M. Toussaint in France—has grown a knowledge of various ways in which the contagion of that dreadful disease can be so mitigated that an animal inoculated with it, instead of incurring almost certain death, shall have no serious illness; and the further knowledge has been gained that the animal submitted to that artificial procedure is thereby more or less secured against subsequent liability to the disease. In other words, with regard to that disease, an infliction which sometimes spreads to man from his domestic animals, and one which in some parts of Europe is of serious consequence to agricultural interests, as well as to animal life, the later experimenters—of whom I may particularly name M. Toussaint, and our countryman, Professor Greenfield—seem to be giving to the animal kingdom, and to the farmers, the same sort of boon as that which Jenner gave to mankind when he taught men the use of vaccination. Quite recently our great leader, M. Pasteur, seems to have made, by new experiments, still further progress in the mitigation of anthrax.

Thirdly, a similar discovery has been made by M. Pasteur, with reference to the contagium of a very fatal poultry disease, known by the name of fowl's cholera; he has learnt to mitigate that contagium to a degree in which, if fowls be inoculated with it, they will suffer no serious ailment, and he has found that fowls so inoculated—or, as he, in honour of Jenner, would say, "fowls so vaccinated"—are proof against future attacks of the disease.

Fourthly, Professor Semmer, of Dorpat, through experiments done under his direction by Dr. Krajewski, has made a similar discovery in regard to the infection of septicæmia—has found, namely, that, by treatment like that with which M. Toussaint mitigates the contagium of splenic fever, he can bring the most virulent septic contagium into a state in which it shall be mild enough to serve for harmless inoculations, which inoculations, when performed, shall be protective against future infections.

Finally, in a different direction of experimental work, let me name the recent most admirable research which Dr. Schüller, of Greifswald, has made, nominally in respect to certain surgical affections of joints, but in reality extending to the inmost pathology and therapeutics of all tubercular and scrofulous affections. A knowledge of the fatal infectiveness of crude tubercular matter has been given (as I before said) by Villemin and those who followed him, and Professor Klebs, four years ago, declared the infective quality to be due to the presence of a microphyte (micrococcus), which he had succeeded in separating from the rest of the matter by successive acts of cultivation in fluids of inorganic origin. Dr. Schüller solidly settles, and widely extends, that teaching. According to his, apparently quite unquestionable, observations and experiments, the micrococcus which characterizes tubercle characterizes also certain affections popularly called "scrofulous"—namely, "scrofulous" synovial membrane, "scrofulous" lymph-glands, and lupus; so that these diseases may be defined as essentially tubercular, and that inoculation with matter from any of them, or with a cultivation-fluid in which the micrococcus from any of them has been cultivated, will infect with general tuberculosis. The rapid multiplication of the tubercle-micrococcus in the blood and tissues of any inoculated animal can be verified both by microscopical observation and by inoculative experiment; and an extremely interesting part of the research in explanation of certain of our human joint-diseases is the demon-



stration that if in the inoculated animal a joint is experimentally injured, that joint at once becomes a place of preferential resort to the micrococcus which is multiplying in the blood, and becomes consequently a special or exclusive seat of characteristic tubercular changes. Even thus far the practical interest of Dr. Schüller's book is such as it would not be easy to overstate, but still greater interest attaches to the last chapter of the book, in which, confidently resting on the pathological facts which I have quoted, he makes proposals for the treatment of tubercle on the basis of its microphytic origin, and shows the successful result of such treatment as he has hitherto tried from that basis on animals artificially infected by him.

I venture to say that in the records of human industry it would be impossible to point to work of more promise to the world than these various contributions to the knowledge of disease, and of its cure and prevention; and they are contributions which from the nature of the case have come, and could only have come, from the performance of experiments on living animals.—*Lancet*, Aug. 20, 1881.

### Hospital Notes.

BELLEVUE HOSPITAL, NEW YORK.

(Service of Dr. AUSTIN FLINT, Sr.)

(Specially reported for the MEDICAL NEWS AND ABSTRACT.)

#### *Chronic Nephritis. Uræmic Convulsions (Unilateral). Phthisis Pulmonalis.*

Jennie C., æt. 40, Irish, domestic; admitted Sept. 20, 1881. About sixteen months since the patient had the initial lesion of syphilis, which was followed by secondary manifestations of the same disease. She was subjected to a course of treatment with mercurials, under the use of which the syphilitic symptoms disappeared in Aug. 1880. From that time until May, 1881, she enjoyed good health, having no prodromata of any disease whatever. At that time she suddenly became unconscious, and, falling to the ground, had several convulsions. Recovering from these, she pursued her ordinary avocations without applying for medical aid. Since then, however, she has had a convulsion almost every week, and sometimes twice a week. She has often bitten her tongue and foamed at the mouth during the convulsions, which have, however, been of brief duration, continuing only a few moments at any one attack. On Sept. 19, 1881, she had an unusually severe and protracted convulsion.

On admission (September 20, 1881), she was somewhat stupid, but conscious, and complained of loss of power in her left side. Suddenly, while undergoing examination, she became convulsed, the left side of the face and the extremities of the same side being most affected. The eyes were fixed, and blood-stained froth came from the mouth. Paroxysms similar to that just described recurred at short intervals during the night and on the following morning.

Physical examination revealed the second stage of phthisis pulmonalis at the left apex. The urine was acid and clear, had a specific gravity of 1021, and contained ten per cent. of albumen by volume, together with large and small hyaline and granular casts. The quantity of urine in twenty-four hours was ten fluidounces. Two drops of *Ol. tigii* were administered, inasmuch as another uræmic convulsion was apprehended, and produced a small fecal evacuation.

September 21. The patient is still stupid, and has slight *subsultus tendinum*. One-tenth of a grain of elaterium was now exhibited every three hours until

three doses had been administered. The result of this medication was to produce several watery movements and to relieve the ataxic symptoms.

22d. The amount of urine for the past twenty-four hours has been only eight fluidounces. The patient had one attack of eclampsia in the morning, followed by profuse diaphoresis. She was placed upon the use of infusion of digitalis, one-half ounce, and bitartrate of potassium, one drachm, every three hours.

23d. Patient had two violent convulsions during the night, but soon completely recovered from them. Urine in twenty-four hours, sixteen fluidounces.

24th. One convulsion occurred during the night. Urine passed, thirty-six ounces. The same treatment is continued.

25th. Amount of urine, forty-seven ounces. No convulsion occurred.

26th. No eclampsia. Amount of urine, forty-two ounces. Appetite has returned.

28th. Amount of urine, thirty-seven ounces. It is clear, has a specific gravity of 1025, and contains albumen, 5 per cent., by volume, with a few granular and hyaline casts. The digitalis was discontinued.

October 8. No convulsions have occurred since last note. Patient's general condition has improved. Urine, forty ounces, and contains no casts, although some albumen still remains.

12th. The patient is so well as to desire her discharge.

#### *Recurrent Fibroid Mammary Tumour.*

An interesting case of this kind recently came before Mr. GAY, at the Great Northern Hospital. He had removed the mammary gland a twelve-month previously. There was no axillary complication, and the wound healed well in about three to four weeks. The patient was a lean person, aged fifty-four years. When she presented herself again, two painful growths had recurred on the axillary side of the site of the former operation, and had ulcerated on their surfaces. The bases were of almost a scirrhous hardness, but unadherent to the subjacent structures. The ulcerated surfaces were flattened, and had the appearance of being made up of eversion, from central gaps of the skin and, might be, allied structures.

Mr. Gay said he deemed the case one of considerable interest, as illustrating a by no means uncommon instance of fibro-recurrent or recurrent adenoid growth, such as he had described repeatedly in the *Pathological Transactions*, and which has a history, both clinical and pathological, of its own. Mr. Gay had no difficulty in distinguishing the case from absolute cancer, although it appears to stand close on the immediate confines of that disease. Originally a growth from the mammary fibrous structures, its recurrence is never prevented by the ablation of the gland. It is but slightly infective, so that the axillary glands do not show any marked disposition to enlargement, either by contamination or sympathy; and in the event of their becoming enlarged, the swellings usually either subside, or ulcerate after the fashion of the parent growths. It is at certain stages spindle-cell shaped in its histological characteristics. The tendency of these tumours to grow, as well as to ulcerative degeneration, is in the direction of the skin. They admit of removal in any stages of their career, and certainly with much temporary advantage to the patient, as the disease seems to have a specifically local character, and does not usually invade vital organs. On the other hand, with removal, the patient is very prone to improve in constitution, and to get healthy and fat. Such cases must have come under the observation of other surgeons; but, as far as Mr. Gay was informed, have not met with much attention.—*Lancet*, October 8, 1881.

## MONTHLY ABSTRACT.

## Anatomy and Physiology.

*The Origin of the Liquor Amnii.*

Dr. WIENER, of Breslau, has experimentally investigated upon dogs and rabbits the above subject, with special reference to the part played by the fetal kidneys in the production of the amniotic fluid. His results are communicated to the *Archiv für Gynäcologie*. The amnion is of course a fetal membrane, and therefore it might be supposed that, at least in the early months of pregnancy, the fluid within it was in some way furnished by the fœtus. From the fact that the liquor amnii contains urea, some have supposed that the fœtus passed urine into it. But some experiments which have been made by Kuntz, and confirmed by the author, show that when a solution of an indigo salt is injected into the maternal blood, the liquor amnii becomes coloured by the pigment, although none of it enters the fetal circulation or stains the fetal tissues. And according to Fehling and Ahlfeld, the secretion of urine by the fœtus during intra-uterine life is very scanty and slow. If these facts be correct, it would seem that the liquor amnii in the later months of pregnancy is mainly furnished by the mother, and that fetal urine does not, as a rule, form a part of it. To determine both whether the kidneys act, and whether their secretion does add to the amount of the liquor amnii, Dr. Wiener injected the indigo solution, with a hypodermic syringe, directly into the fœtus. This, he says, is not difficult to do. He found that when introduced thus into the fetal circulation, it was excreted by the kidneys, and tinged the liquor amnii. He concludes, therefore, that the kidneys do actively act in intra-uterine life; that the urine is passed into the amniotic sac; and that the liquor amnii largely consists of fetal urine. His experiments give a little additional support to what might have been inferred from the facts, that, as mentioned, the liquor amnii commonly contains urea, and that in cases of malformation and occlusion of the fetal urethra, the bladder and kidneys are found distended with urine.—*Med. Times and Gazette*, Oct. 8, 1881.

*Heat Dyspnoea.*

Several physiologists have lately turned their attention to the interesting phenomenon known as "heat" or thermal dyspnoea, and a *résumé* of the results obtained is given in the last number of the *Revue Scientifique*. It is well known that when the temperature of the blood increases, the respiratory rhythm becomes more frequent, dyspnoea supervenes, and those phenomena are observed which are properly attributable to exaltation of temperature. The interesting question arises, Where are the points of attack for the increased temperature? Does it act on the cerebral convolutions primarily, and through these on the respiratory centres, or does it act on the skin first, or on the pulmonary endings of the vagus first, or, finally, does it act directly on the nerve-centres which govern the respiratory movements? Goldstein believes he has proved that increased temperature does not act by way of the skin, because in a heated animal cold affusions do not at once destroy its effects; that it does not act through the cerebrum, or is owing to any feeling of uneasiness on the part of the animal, because it is not

prevented by morphia injected into the veins; that it does not act by the pulmonary expansion of the vagi, because it is not prevented by their section; but he endeavoured to show that it is occasioned by the direct action of heated blood on the respiratory nerve-centres by employing a method of increasing the temperature of the blood without exciting the sensory nerves at their periphery. This method consisted in exposing the carotid of a rabbit, and without otherwise disturbing the circulation through it, surrounding it with hot water, which of course heated the blood passing to the brain and medulla oblongata. Goldstein came to the conclusion that as heat dyspnoea then set in, it was clearly due to the action of the warm blood upon the respiratory nerve-centres. M. Gad and M. Mertchinsky have made a series of experiments on rabbits in this mode, and on registering by means of a pneumograph it was found that about thirty seconds after the heating of the carotidean blood the respiratory rhythm became four or five times more frequent, and ten seconds later heat dyspnoea was fully established. The same result was obtained by another experimenter, and in an article in M. Foster's *Journal of Physiology*, contributed by M. Sihler, that observer considered that it was attributable partly to pain and partly to an increase in the amount of carbonic acid contained in the blood, and to the augmented molecular changes naturally consequent on an elevated temperature. But this conclusion is disputed by the most recent experimenters, Mertchinsky and Gad, on the ground that the type of thermic dyspnoea differs from that of asphyctic dyspnoea. In heat dyspnoea respiration is short and shallow, whilst in asphyctic dyspnoea it is deep. According to Mertchinsky moderate doses of chloral do not modify thermic dyspnoea, whilst toxic doses almost entirely prevent it from supervening, or at least cause it to appear at a much later period and in a much less marked form. The section of the two pneumogastrics was without effect. Section of the fifth pair did not prevent it from occurring, but it was much less distinctly marked. Hence he concludes that it is not a reflex action consecutive to excitation of the sensory nerves of the face by heat. Moreover, the respiratory excitement is observed in as distinct a form after ablation of the encephalon as before. Hence Mertchinsky maintains that the type and rhythm of the respiratory acts depend on the temperature of the inspiratory nerve-centres situated in the medulla oblongata.—*Lancet*, Oct. 8, 1881.

## Materia Medica and Therapeutics.

### *Absorption of Mercury.*

AFTER the inunction of the ointment of mercury in rabbits and men, FURBRENGER failed to discover any trace of mercury in the rete Malpighii of the skin, but globules were visible in the hair-follicles and in the sweat-ducts. If, however, the epidermis was first removed, the metal was then found in the corium, but only a few isolated globules could be seen in the deeper layers of the skin and in the subcutaneous tissue. After exposure to mercurial vapour no mercury could be found in the hair-follicles or sweat-ducts, although the surface of the skin was covered with a gray deposit. Twenty-four hours after an injection of a mercurial emulsion into the jugular vein the presence of dissolved mercury in the blood could be demonstrated in five out of eleven experiments, and the liver always contained mercury in a state of solution. Hence it seems certain that metallic mercury becomes dissolved in the blood. The metallic mercury in the hair-follicles was considerably diminished in quantity in eight days, and those globules which remained were oxidized on the surface, angular, and dirty-black in colour.

It is probable that the fatty acids of the skin assist in the solution. Oberländer has found that silver could be detected in the urine 190 days after the cessation of a course of inunction. The excretion was not, however, uniform, but presented remissions and exacerbations, the former occasionally of ten days' duration. A course of treatment by sulphur waters seemed to make no difference to the excretion, and no influence could be ascribed to salt baths, soda baths, or vapour baths. He concluded from his experiments that mercury is probably excreted largely by the liver and intestine. Although the action of mercury has been much studied, little attention has been given to the lesions which are found when death results from its action. Mering has produced in cats all the symptoms of chronic mercurial poisoning by injecting daily a solution of oxide of mercury in glycochol—stomatitis, salivation, anorexia, diarrhoea, anaemia, prostration, albuminuria. After acute poisoning the autopsy uniformly revealed intense hyperaemia of the gastro-intestinal mucous membrane, and often actual erosions. The lesions were most intense in the lower part of the small intestine. Occasionally hemorrhages were found in the cardiac muscle, and ecchymoses in the bladder. The liver and kidneys were intensely congested. After chronic poisoning there was found a similar inflammation of the gastro-intestinal mucous membranes, ulceration in the small intestine, and a still more severe affection of the large intestine. Ulceration in the mouth and necrosis of the maxillary bones were also common. These effects are regarded less as due to a general cachexia than to the localized action of mercury on the different organs.—*Lancet*, Sept. 24, 1881.

#### *Carbolic Acid as an Antipyretic.*

At a recent meeting of the Société de Biologie, M. RAYMOND gave an account of the markedly antipyretic qualities possessed by carbolic acid, which he has administered in cases of typhoid fever. The acid is given simultaneously by mouth and rectum, to the amount of one gramme per diem, fifty centigrammes being divided into three pills, and a like quantity used in enemata. He declares that its administration on this plan is very soon followed by a great reduction in temperature—viz., to the extent of  $3^{\circ}$  or  $4^{\circ}$  C., and within half an hour most profuse diaphoresis is established. To ascertain whether this hypothermic effect was produced by the direct action of the carbolic acid, or indirectly through the diaphoresis, M. Raymond suppressed the latter by the injection of duboisin, and found the lowering of temperature to be unaffected. He found also that the antipyretic action was increased the longer the administration was continued. By employing larger doses—viz., two grammes, toxic effects were produced, the temperature fell to  $34^{\circ}$ , convulsive tremors occurred, and carboluria appeared. At the suggestion of M. VULPIAN, carbolate of soda was given in doses of one gramme and a half, instead of the free acid, and the patient kept under the influence of the drug. The toxic effects did not appear, but the antipyretic action was still marked. The remedy does not, according to M. Raymond, limit the duration of the stages of typhoid fever; it controls the pyrexia of idiopathic erysipelas in a similar way, but has no influence on that of tuberculosis. It was pointed out that the antiseptic could not, of course, pass through the alimentary canal unchanged, so as to reach the ulcerated small intestine, and it would seem simply that it acted through the spinal cord and the vascular system.

M. HALLOPEAU thought the acid might have a topical effect on the typhoid ulcers. M. DUMONT-PALLIER pointed out that it was not so good a regulator of the fever as refrigeration. M. HANOT made some curious observations, which he detailed more at length at the next sitting of the society. He said that in two



cases of adults suffering from typhoid fever, whom he had treated by carbolate of soda given internally, defervescence was determined on the fourteenth day in one case, and on the sixteenth in the other, the temperature falling  $3^{\circ}$  to  $4^{\circ}$  C. on the second or third day of treatment. In each case a peculiar rash appeared at the same time as the fall in temperature, the rash becoming pustular and varioliform in a short time. The pustules, on microscopical examination, were found to be formed almost wholly of bacteria mingled with a small amount of pus. The urine also contained bacteria. M. PAUL BERT, in commenting upon this, went so far as to say that carbolic acid renders the organism unfit for the development of the bacteria, and causes the particular material in which they thrive to be eliminated by the skin and kidneys, and the bacteria following its elimination are found in these parts. Upon what slender facts are marvellous and weighty theories sometimes raised!—*Lancet*, Aug. 20, 1881.

#### *Merits of Hospital Kwass.*

This, an old popular Russian drink, is included in the obligatory diet of the military hospitals in that country, being given out (in full diet) in allowances nearly one litre. Iljinsky, at the request of his senior medical officer, undertook the task of overlooking the preparation of the beverage, and daily analyzing it as to its contents of acids, alcohol, and extractive matter. For the preparation of 5496.161 litres (80 eimers), the following ingredients are made use of: Barley malt, 104.950 litres (4 tschetwerik); rye malt, 1638.4 to 4095.10 grammes (4 to 10 Russian pounds); rye meal, 24,570.60 grammes (60 Russian pounds); yeast, nearly  $\frac{3}{4}$  of a litre ( $\frac{3}{4}$  of a "krug"); peppermint, 2047.55 grammes (5 Russian pounds); wheat meal, 819.2 grammes (2 Russian pounds). When brewed under observation, the specific gravity was 1012 to 1021, although formerly it was 1004 to 1006, and Dr. Georgievski gives 1006 to 1007 as the average specific gravity of kwass. Hence it appears that in general 70 per cent. of the officially determined quantities of ingredients are, for economical reasons, not used. The analysis showed it to contain a series of valuable nutritive constituents, thus: lactic acid (according to Preyer a nerve-sedative), acetic acid (a thirst quencher), and, further, alcohol and carbonic acid, both of which hinder tissue change, and thus, to a certain extent, lower the temperature. The day's hospital ration of kwass contains: albuminates, 5.5 grammes; carbon hydrates, 33.0 grammes. The merits of the drink may be thus summed up: it excites appetite, acts both on the nervous system and on tissue-change, and is to a certain extent nourishing. When it is rapidly cooled after boiling, as is done with beer, a beverage resembling stout, but of more nutritive value, is obtained.—*London Med. Record*, July 15, 1881.

#### *Fernet's Method of Introducing Food and Medicine by the Nostrils.*

M. FERNET (*Revue de Thérap.*) wishes to popularize the method of introducing liquid or semi-solid elements, and certain drugs, by the nostrils. The author has seen it employed successfully in newly-born infants, too weak to take the breast or milk from a spoon. He proceeds as follows: The patient being laid on his back, a little raised, the end of a spoon or, better, the spout of a close vessel, is brought near to one nostril, and its contents are poured in gently at intervals. The liquid slides over the floor of the nasal fossæ and the roof of the palate, and reaches the pharynx, where it induces movements of regular deglutition. If the operation be well done, the liquid never returns by the other nostril. This method may be applied in certain cases of apoplectic coma, when the patient

cannot drink for three or four days successively, in the tuberculous meningitis of children, etc.—*London Med. Record*, July 15, 1881.

#### *Preparations of Digitalis.*

DR. FRANKEL (*Charité-Annal.*, Band vi.) says, that the leaves of digitalis have an effect upon the circulation, which differs greatly according to their source, and possibly also to their freshness. The German Pharmacopœia prescribes a watery extract prepared from the pressed leaves; an alcoholic and an ethereal extract; and, finally, one obtained with the help of vinegar. The tincture is made by treating crushed portions of the fresh plant with six parts of alcohol; while the acetum is obtained by maceration of the dry leaves with a mixture of one part of alcohol and nine parts of vinegar. About ten grammes of the tincture are considered equal to one part by weight of the leaves of digitalis. Digitalis, when given in small doses, has a very different effect from that obtained by large quantities. The latter are chiefly administered in cases of acute inflammation, attended with fever, while the former are employed to remove the troubles observed in heart-disease, in the so-called stage of compensatory disturbance. While the effect of smaller doses (0.5 to 0.75 gramme daily) is retardation of the contractions of the heart with increased arterial tension, larger doses (1.0 to 1.5 gramme daily) are followed by diminished arterial expansion and reduced frequency of the pulse, not unfrequently accompanied by collapse and vomiting. These latter doses were therefore administered as an antiphlogistic and antifebrile in pneumonia, pleurisy, pericarditis, and acute rheumatism, especially when the disease was naturally inclining towards a crisis and recovery. Traube explained this influence of digitalis in reducing temperature by its action upon the vaso-motor nervous system, and the vascular expansion of the skin especially. Traube's experiments have shown that the smaller doses in heart-diseases, by exciting the vagus nerve, retard the beating of the heart, and, by increasing the pressure of the aorta, regulate the adduction of a sufficient quantity of arterial blood to the myocardium, so that this organ recovers from its previous state of exhaustion, and is once more enabled to contract energetically. Only when the poisonous effect increases, paralysis of the cardiac nervous system and sinking of the blood-pressure set in. Böhm has since proved, by experiments on the exposed hearts of frogs, that digitalin has a specific effect upon the cardiac muscle itself, and upon the terminal branches of the vagus nerve connected with it, whereby the remarkable phenomena in the action of the heart are chiefly called forth. In order to test the activity of the different preparations of digitalis used in practice, dogs of middle sizes were curarized and injected subcutaneously with the infusion, the tincture and the acetum (the alcohol and vinegar having been removed), and the beginning of the retardation of the pulse and the enlargement of the single beats were considered as the commencement of the action. The following were the results. 1. The tincture, and also the acetum and the infusion of digitalis, are capable of exercising an exciting influence upon the inhibitory nerves of the heart, and upon the vaso-motor centre; but there are unmistakable grades of distinction in the action of the three preparations. 2. The tincture is the least reliable. 3. The action of digitalis—retarding of the pulse and increased blood-pressure setting in, after divers large doses of both the infusion and the acetum—sets in according to the individuality of the animals, and other accessory circumstances. 4. The acetum digitalis is the most powerful.—*London Med. Record*, Aug. 15, 1881.

*Dangers of Peritoneal Transfusion.*

Professor MOSLER (*Deutsch. Archiv für Klin. Med.*, vol. xxviii.) refers to the successful cases of Ponfick with injection of defibrinated blood into the abdomen (already recorded in the *London Medical Record*, vol. viii. p. 12), and to the observations which we have also recorded of Golgi and Bizzozero (*Ibid.*), indicating the excellent effect of such injections in increasing the richness of the blood, as tested by observations on the increase of blood-corpuscles and hæmoglobin. Mosler assumes as an indication for transfusion the leukæmic quality of the blood; and the observations of himself and others on the effect of transfusion in leukæmia indicate that it should be done repeatedly in similar cases. It could not as yet be repeated often enough, on account of the dangers and difficulties of transfusion into the vessels. It being reported by Ponfick and Bizzozero that peritoneal transfusion presented greater advantages, the author resolved to try it on his patients, and especially to study the influence of repeated transfusion in the same patient. The first patient who appeared adapted for peritoneal transfusion was a man, aged 27, who had had sixteen parenchymatous injections of Fowler's solution of arsenic into a chronic tumour of the spleen without any symptoms of irritation of the peritoneum. Peritoneal transfusion appeared to be indicated by the anemia, which had set in in a very high degree; besides, it was of especial interest to be able to state whether the splenic tumour, which had considerably diminished after the injection of Fowler's solution, would show a yet greater diminution. After repeated peritoneal transfusion, microscopic examination of the blood showed, besides a diminution of the red corpuscles, a large number of microcytes. The origin of the tumour was referred to intermittent fever and syphilis. On December 8, 1880, 40 centigrammes of defibrinated blood were slowly injected, by means of a well disinfected injecting apparatus, through a puncture made in the left side of the abdominal wall. On the place of puncture a carbolized compress was laid, fastened with a gauze bandage; and above this a bag of ice was placed. In the morning and evening the patient received a dose of 15 milligrammes (nearly a fourth of a grain) of pure opium. He complained of pains in the place of puncture, both during the operation and for some time afterwards. The temperature in the evening was 38.4 deg. Cent. (101 deg. Fahr.); the following morning 37.2 deg. Cent. (99 deg. Fahr.); no alteration could be noticed in the urine and blood. On December 21, the peritoneal transfusion was repeated with the same apparatus. The place of puncture was the linea alba, about 7 centimetres below the navel. The transfusion, of 130 cubic centimetres of blood, was done gradually. The patient complained while the puncture was made, and also afterwards, of pains in the place of puncture, and upwards. Hourly measurements, after the transfusion, showed a moderate elevation of temperature. The pulse was 110 per minute. In the night the pains increased. On December 21, in the morning, the temperature was 39.6 deg. Cent. (103.3 deg. Fahr.), pulse 128; there were symptoms of peritoneal irritation. The abdomen was distended; he had considerable difficulty in breathing; the heart's action became irregular; and death occurred on the following night. The *post-mortem* examination showed diffuse peritonitis, a tumour of the spleen, and a high degree of syphilis of the liver. In this case, observation shows that peritoneal transfusion cannot be recommended in all conditions of the vascular system. The author suspects that the repetition of the transfusion was the cause of the peritonitis; since neither were the instruments dirty, nor was there any injury to the intestine, nor abnormality of the injected blood. In this case an interval of twelve days occurred between the two operations, in which the patient showed no signs of peritonitis. He is inclined

to think that the immediate cause of peritonitis here was an increased vulnerability of the peritoneum, due, probably, to the previous transfusion, as well as to the disease from which the patient suffered. He points out that peritoneal transfusion at short intervals must not be regarded as a harmless operation; and that it is probably contra-indicated in inflammatory and other diseases of the abdominal organs attended with a tendency to peritonitis.—*London Med. Record*, Aug. 15, 1881.

#### *New Method of Producing Anæsthesia of the Larynx.*

Prof. ROSSBACH, in the *Ann. des Maladies de l'Oreille, du Larynx, et des Organes Connexes*, Sept. 1881, describes his method, which consists in an attempt to suspend the conductivity of the trunk of the sensory nerve of the larynx so as to produce complete anæsthesia of that organ. The trunk of the sensory branch of the superior laryngeal nerve reaches the interior of the larynx by penetrating the thyro-hyoid membrane, below the extremity of the greater horn of the hyoid bone. At this point the nerve trunk is very superficial, and it is very easy, by means of ordinary agents, to destroy its conductivity. The author uses subcutaneous injections of morphia, 0.005 grm. at this point on both sides of the neck. Success was complete. He also found, by experiments made on healthy subjects, that the conductivity of this nerve could be suspended by cold. He used for this purpose a Richardson's atomizer, with two jets so arranged that the spray is thrown on both nerves at the same time. A spray of ether served in less than two minutes to render the interior of the larynx entirely insensible of contact with a foreign body. The author thinks that this method might be of use in cases of reflex spasm, where the point of departure is in the interior of the larynx, as well as in painful affection of this organ.

### Medicine.

#### *Presence of Bacilli in Typhoid Fever.*

As a conclusion of his earlier communications on this subject, EBERTH reports the results of the examination of seventeen cases of typhoid fever. For the sake of comparison, he contrasts these cases with eleven examples of different forms of infection, in which, with the exceptional occurrence of micrococci in the lymphatic glands, no bacilli were found, and with thirteen cases of tuberculosis and phthisis, in which, in spite of the extensive ulceration of the intestinal canal, no allied forms of organism were found in the spleen or lymphatic glands. The ulceration of the intestine did not here, as in typhoid fever, favour the introduction of organic forms. Among the cases of typhoid fever, bacilli were found in six instances, especially in the infiltrated lymphatic glands, more seldom in the enlarged spleen, and in eleven cases they were absent. In the cases in which these organisms were found, as in his earlier observations, the duration of the disease was, as a rule, larger; the number of bacilli, however, with the exception of one case of fourteen days' standing, in which they were found in great abundance, was less. The bacilli agreed in their general characteristics with those previously described by the author and by Klebs; in addition, however, to the usual forms, longer and broader threads were noted, which were perhaps only other developmental forms.—*Centralblatt f. d. Med. Wissenschaft*, Sept. 10, 1881.

*Treatment of Typhoid Fever by Salicylate of Soda.*

M. CAUSSIDOU made a communication to the meeting of the French Association for the Advancement of Science at the Congress of Algiers, which was based on thirty-two cases of typhoid fever treated by salicylate of soda, and in which the rise of the temperature and the influence of this drug on the febrile process had been registered with the greatest care, as attested by numerous tracings shown by the writer. M. Caussidou arrived at the conclusion, in opposition to the facts observed in several wards of the Paris hospitals, that salicylated medication gives larger, more certain, and more permanent effects than refrigeration. M. Caussidou has even been in doubt if, by administering salicylate of soda from the outset of typhoid fever, it would not be possible to limit the duration of the disease to the first week (?), and if, at least, it would not be possible to obtain a number of cases belonging to the abortive form. Nevertheless, M. Caussidou does not conceal the dangers of salicylate medication. Like other observers, he has noted dyspnoea, precordial trouble, and exhaustion in patients where the salicylate of soda brought on a too sudden apyrexia. To avoid these objectionable results, he proposes to administer salicylate of soda in fractional doses of one gramme given every two hours, and to stop as soon as the temperature falls below 38° Cent. (100.4° Fahr.) In a complicated case of erysipelas, the salicylic medication was powerless to produce a febrile recrudescence brought on by this complication. M. Hérard declared that he had nothing but commendation for the use of antiseptics, such as carbolic and salicylic acids, in the treatment of febrile diseases.—*London Med. Record*, July 15, 1881.

*Period of Latent Muscular Excitation in Different Nervous Diseases.*

This paper (*Archives de Physiologie*, No. 2, 1880) is a continuation of MENDELSSOHN's previous communication (*Travaux du Laboratoire de M. Marey*, 1880), in which he had given the results obtained by him in the physiological laboratory from experiments on frogs and healthy human subjects. The main conclusion which he had reached in the latter was, that the time of latent excitation is not constant, but varies even in the same subject, with variable conditions of nutrition, rest, amplitude of muscular curve, strength of exciting current, etc. In the frog it averages .008 second; in man, .007. Generally speaking, the duration of latent stimulation bears an inverse ratio to the muscular excitability and contractility. Experiments in diseased conditions bear out these physiological data. There is a diminution of the lost time in hemiplegic late rigidity, an increase in hemiplegic muscular atrophy, and so on in the whole list of diseases with tendency either to contracture or to atrophy. The author found considerable variations prevail in hysteria, but noted that shortly before an attack the lost time was shorter. In artificial catalepsy, it sunk to .001 second. [Waller (*Brain*, No. 10, 1880), in a recent paper, assumes the time of latent contraction of healthy human muscle to be .02 second, and gives tracings in support of his view. It will be remembered that Helmholtz (1852) had estimated the same period (in frogs) at .01 second, and that his determination has since been generally accepted and reproduced in text-books.—*Rep.*]—*London Med. Record*, June 15, 1881.

*Spinal Myosis and Reflex Pupillary Rigidity.*

Prof. W. ERB (*Centralb. für die Med. Wiss.*, No. 17, 1881) has found that, out of 84 cases of tabes dorsalis, 59 had absolute reflex pupillary rigidity, 12 showed very feeble and low reaction to light, and in 13 the reaction was normal.



In 5 cases the symptom was unilateral, in 7 there was more or less advanced atrophy of the optic nerve. Out of the 71 cases in which more or less rigidity was present, 37 had distinct myosis, 34 had normally wide pupils, sometimes unequal. Myosis was absent in quite half the cases. Of the 71 cases, 43 were in the initial stage; in the remaining 28 the ataxy was already manifest. These symptoms may therefore appear quite early in the disease, or, on the other hand, be absent after it has existed for years; a closer connection between syphilis and pupillary rigidity appears not to exist in tabes. Out of 16 cases of progressive cerebral paralysis, he found the pupils only twice normal; in 10 they were unequal, but of these only 3 showed rigidity; in 4 cases there was myosis combined with rigidity. All this holds good also of the earlier stages of the disease. Very much more often were pupillary differences observed, 10 times in 16 cases; the reflex rigidity, on the other hand, only 7 times (once unilaterally). This symptom was also noticed several times in other diseases (anæmia, syphilis, tumours); in myosis after disease of the cervical sympathetic, the reaction to light is generally preserved, as well as in lesions of the cervical part of the spinal cord. Myosis and rigidity do not appear to occur together in healthy persons; in uncomplicated senile myosis, the reaction to light is unaffected. They must, therefore, be regarded separately. Respecting the former, it is most probable that the pupil-dilating centre, or its tract in the spinal cord or in the cervical sympathetic, is paralyzed; the rigidity, on the other hand, seems to be due to some change in the reflex arc, which lies between the centres of the optic nerve and the third nerve. Further observations, by irritation of the peripheral cutaneous nerves, showed that not only was there paralysis of the reflex contraction of the pupil, but also of the reflex dilatation, so that the expression "reflex rigidity" is doubly correct.—*London Med. Record*, July 15, 1881.

—  
*Magnet, Static Electricity, and the Tuning-Fork, in Pseudo-Syphilitic Paralysis.*

DRS. CH. MAURIAC and R. VIGOUROUX (*Le Progrès Méd.*, 1881, No. 19, 21, and 22) have published two examples of what they describe as a rare form of nervous disorder occurring during the course of syphilis, but of which it is difficult to determine exactly what part syphilis plays in their determination. The first case was that of a man, a haircutter, aged 22, who had chancres, followed by roseola, crusts on the scalp, sore mouth, with headache, fever, and night-sweats. After treatment for two months he was discharged cured, but soon returned with a painful tumour over the inner aspect of the arm, along the inner border of the triceps; under large doses of iodide of potassium this rapidly disappeared, but the limb became anæsthetic, with loss of motor power, and preservation of normal electrical reactions. This did not improve under iodide; but the application of a magnet caused headache, and complete transfer of the anæsthesia and paralysis to the sound arm; repeated applications cured these for a time, but he relapsed after a few days. Static electricity, repeated three times, produced no effect but slight numbness of the fingers and dorsum of the hand. By introducing the sound hand into the resounding box of a large tuning-fork (ut 3), transfer of the affection was effected, and at the same time there was produced hemianæsthesia of the sound side, with loss of taste, smell, hearing, diminution of the field of vision and visual acuity, and complete colour-blindness; in every respect this resembled hysterical hemianæsthesia. By the following day the phenomena had changed sides, even the artificially induced hemianæsthesia having become transferred. By repeated applications, a cure was effected: the patient went home, relapsed again, was put on iodide of potassium, and after taking it for some time,

got well suddenly, and has remained well. The second case was that of a man, aged 40, who denied all venereal antecedents, and the diagnosis of constitutional syphilis rested on the presence of a fissured and atrophied tongue. He had been epileptic when young, and his mother suffered from the same disorder. After suffering from diffuse superficial headache, worse at night, he became hemianæsthetic, the anæsthesia affecting the special senses, with some loss of power in the lower extremity. The magnet and tuning-fork caused malaise and insomnia, but had no influence on the neurosis. Statical electricity caused transfer, and cured him after twenty sittings of forty to forty-five minutes each. The authors admit that the evidence of syphilis was not clear, but the lesion of the tongue was regarded as syphilitic, both at the Hôtel-Dieu and at St. Louis. The phenomena were not those of ordinary syphilitic paralysis, yet a localized lesion in the posterior part of the internal capsule would be capable of giving rise to these symptoms. The early epileptiform attacks in the first case, and the existence of hystero-epilepsy in the mother of the second, indicate that in both there was a predisposition to nervous affections. The inefficacy of ordinary antisyphilitic remedies is opposed to the notion that these symptoms depended directly on syphilitic lesions; but syphilis probably played the part of an exciting cause.—*London Med. Record*, July 15, 1881.

#### *The Cephalic Souffle in the Adult.*

In 1838, Dr. Fisher, of Boston, published in the *American Journal of the Medical Sciences*, a paper in which he described the *bruit de souffle* in the head, and stated that he had met with this sound in auscultation of the cranium in cases of chronic hydrocephalus, cerebral congestion, either simple or coincident with dentition or whooping-cough, in acute encephalitis or meningitis, in suppuration of the brain, induration of that organ, etc. Other authors recognized the same sound later, and reported it with other affections; among others, M. Henri Roger, who found it only exceptionally after the closure of the fontanelles, and expressed the opinion that cranial auscultation is not really applicable to persons past the first two or three years of life. Subsequent writers to M. Roger have, as a rule, agreed with him in this opinion, though it has, perhaps, not been altogether denied that the cerebral *souffle* might occur in the adult also.

M. RAYMOND TRIPIER, in a memoir published in the *Revue de Médecine* (the continuation of the *Revue Mensuelle*), Nos. 2 and 3 of this year, takes up the subject anew and reports six cases of the occurrence of this intracranial *souffle* in the adult, with a very thorough discussion of the conditions of its occurrence and its significance. The following are the conclusions of his memoir:—

1. The cephalic *souffle* occurs in the adult as Fisher and Whitney have stated, and, contrary to the opinion of M. Henri Roger, now generally accepted.

2. I have met with it in one case of anæmia from neuralgia, in several cases of chlorosis, in one patient suffering from cachectic anæmia, in one case of intracranial tumour, and in a case of hydrocephalus.

3. It is a profound systolic *souffle* that can be heard over the whole cranium, but principally over the lateral portion at the horizon of the temples; its maximum intensity is in the right temporal region, and it does not appear to be modified by changes of position of the head and trunk.

4. The patients in whom it occurs have no intermittent sound synchronous with the *souffle* heard on auscultation, and, consequently, with the cardiac systole, the intensity of which is in direct relation with that of the cephalic *souffle*.

5. Both this subjective sound and the *souffle* may be modified or suppressed momentarily by the compression of the carotid on the side auscultated, or even that of the opposite side. Simultaneously we observe in the anæmic patients the

production of a general *malaise*, with numbness of the hand of the side opposite the compressed carotid. These phenomena are most marked, or are only produced by compression of the right carotid.

6. The cephalic *souffle* may be diminished or disappear with a cure or an aggravation of the disorder which it accompanies.

7. The cephalic *souffle*, being perfectly synchronous with the carotid systole, ought to have its origin in the arterial system. It is not due to a transmission of the systolic *souffle* of the heart that we observe in anæmic or chlorotic patients, nor to that of a *souffle* occurring in the arteries or veins in the neck. By exclusion, we locate it in the terminal portion of the internal carotid, at the point where it enters the cranial cavity. Not only are there many reasons militating in favour of this location, but in one case there was found a small tumour, situated alongside the artery at this horizon, which gave rise to a sound altogether similar to that found in the other cases. The *souffle* may be produced on both sides, or only on one side, and that, preferably, the right.

8. In anæmias due to hemorrhages or to cachexia, as well as in chlorosis, the cephalic *souffle* is met with when the symptoms of anæmia are especially intense and of long duration, notably when there is a very pronounced discoloration of the integuments, palpitations and breathlessness with the slightest exertion, digestive disturbances, and especially vomiting, together with great weakness.

9. In these cases there exists a cardiac systolic *souffle*, which is lacking in cases connected with an intracranial lesion.

10. A cephalic *souffle* without any corresponding sound at the base of the heart, and especially without coexisting anæmia, ought to suggest the possibility of compressions of the internal carotid in its terminal portion, when there is no disease of the orbit.

11. The cephalic *souffle* can be distinguished by the above condition from the continuous *souffle* with reinforcements, which may appear intermittent, produced by communication of the carotid with the cavernous sinus, as well as from the intermittent *souffle* due to aneurisms of the carotid and the ophthalmic arteries, since in both these cases there are characteristic symptoms on the part of the orbit.

12. We have not met with the cephalic *souffle* in the cerebral affections mentioned by Fisher and Whitney, with the exception of hydrocephalus.

13. We have also not found it in the healthy adult.

14. Is there a continuous cephalic *souffle*? We have not met with it in the adult. But the patients may hear sounds that are probably venous *bruits*, either continuous or intermittent, but which must not be confounded with those accompanying the cephalic *souffle*.

15. The cephalic *souffle* may afford important indications for the diagnosis, prognosis, and the treatment of the disease in which it occurs.

#### *Parotitis as a Complication of Ovariectomy.*

Dr. MÖRICKE, in a communication to the *Zeitschrift für Geburtshülfe und Gynäkologie*, narrates five cases in which inflammation of the parotid gland followed ovariectomy, and in four of them went on to suppuration. He refers to the well-known connection between inflammation of the testis and of the parotid gland, and quotes cases from other authors in which affections of the female genitals—swelling of the labia, vulvo-vaginal catarrh, swelling and pain in the breasts, swelling and pain of the ovary—came on in the course of mumps. He thinks these instances point to a connection between the parotid gland and the ovary, similar to that which exists between the parotid gland and the testicle. In sup-

port of this view he further states that he has never seen parotitis follow any other operation on the female genitals, although the operations of this kind which he has done far exceed in number his ovariectomies. In one of his cases there was the possible source of fallacy, that some children suffering from mumps were in the hospital at the time, and a nurse caught the disease. But the ovariectomy case was kept separate from the other patients, had her special nurse, and no other patients in the hospital caught the disease. The criticism, that parotitis is not uncommon in the course of the acute infectious diseases (typhus, scarlatina, etc.), and in pyæmia, he anticipates by saying that his patients were not suffering from any of these diseases; nor was the inflammation so acute or so dangerous as in the pyæmic form. In time of occurrence and in frequency it closely resembled the orchitis which complicates mumps. The parotitis came on five times out of 200 cases of ovariectomy, and began from the third to the seventh day after the operation. Orchitis in mumps is said to occur once in sixty cases, and to come on, as a rule, about the sixth day.—*Med. Times and Gazette*, Sept. 3, 1881.

#### *Peripheral Temperatures in Lung Diseases.*

ANREP has made a series of observations on fifty cases of lung disease, with a specially constructed thermometer, which may be described as an ordinary clinical thermometer in which the bulb has been flattened out horizontally, and is protected from draughts by a glass bell springing from the sides of the tube below the index. With two such instruments accurately compared beforehand, he observed (*Verhandl. der Physik. Med. Gesellsch. in Würzburg*, Band. xiv. Heft 1, 2) the temperature on opposite sides of the thorax of healthy individuals. He found the average to be about 96.3 deg. Fahr.; but the temperatures were very rarely the same, the difference varying from 0.2 deg. Fahr. to 0.5 deg. Fahr., sometimes on the one side, sometimes on the other. The cases of lung disease observed were pleurisy, croupous pneumonia, and chronic catarrhal pneumonia. In pleurisy the cutaneous temperature of the affected side (thermometer in an intercostal space) is higher than the axillary temperature of the opposite side, and the highest temperature corresponds to the time of most rapid effusion. The cutaneous temperature of the opposite side is also slightly raised. In pleurisy without effusion, the rise of cutaneous temperature is neither so great nor so long continued. In cases of croupous pneumonia, the difference between the two sides amounted to as much as 2.7 deg. Fahr.; and from the tables given by Dr. Anrep it can be seen that those parts of the lung in the first stage of inflammation, before crepitation has appeared, give a higher temperature than those completely solidified. In chronic catarrhal pneumonia, the cutaneous temperatures closely agree with the local progress of the disease; and in one case fully recorded the rapid rise of temperature over the apex, followed by a fall as the symptoms of a large cavern developed themselves, is very striking, more especially as the temperature over the cavern continued lower than all the rest of the chest. The author believes that the observation of cutaneous temperatures is, even at the present, of practical value; and when the clinical thermometers now in use are replaced by thermomultipliers, we shall be able to derive from it most valuable information.—*London Med. Record*, July 15, 1881.

#### *Typhoid Pleurisy.*

In certain very rare cases, acute pleurisy may take on a typhoid form which is extremely serious, and almost always ends fatally. Dr. A. GOUJY, who has studied this disease very carefully, thus summarizes in his inaugural thesis the

indications for treatment (*Revue de Thérap.*, p. 331). To keep up the general condition of the patient by hygienic measures, tonics, in large doses, given at an early stage, are the most urgent indication. Subsequently, the pleural liquid must be evacuated as early as possible. One puncture is made, then a second, and if the general condition do not improve, pleurotomy is resorted to. Professor Bouchard is greatly in favour of this treatment; two out of three cases, under his care, recovered under this treatment. The incision of the thoracic wall disengages the lung, and favours its gradual and progressive expansion. Antiseptic washings-out prevent any danger of putrid infection; for this purpose carbolic acid, sulphate of zinc, a three per cent. solution of salicylic acid which has been employed by Potain, and a five per thousand of chloral which has been employed by Villemin, are all recommended. According to the recent statistics compiled by Robert, pleurotomy has yielded 78 cures out of 114 cases. These figures are satisfactory, especially if the frequency of symptomatic pleurisy be taken into account.—*London Med. Record*, Aug. 15, 1881.

—

*Favourable Influence of Hydro-Pneumothorax upon the Progress of Pulmonary Tuberculization.*

This paper (read at the meeting of the French Association for the Advancement of Science at Algiers by Dr. HÉRARD, Physician of the Hôtel-Dieu) was intended to show that, whereas ordinarily the occurrence of hydro-pneumothorax in the course of phthisis forms a serious and often fatal complication, in a certain number of cases, more frequently than is supposed, it exercises a favourable influence on the progress of the malady, may arrest its progress, and become an unhopd for means of safety to the patient. Dr. Hérard had collected a certain number of observations, which established in the clearest and most positive manner this salutary influence of hydro-pneumothorax. An illustrative case was that of a youth, a mechanic, aged 17, who was admitted into the Hôtel-Dieu on July 15, 1880. His mother died of chest-disease, and his father from tuberculosis. He had been delicate from childhood, and had suppuration of the right ear since three or four years of age. Since a sudden chill in May he had had a constant cough, with expectoration, which became muco-purulent. Loss of strength and appetite, sweating, and profuse diarrhoea followed. These symptoms, together with great emaciation and hectic fever, were present on his admission.

Auscultation showed the existence of blowing respiration and subcrepitant *râles* at the right apex. Over the left apex there were cavernous breath-sound and gurgling, especially marked behind. There was dulness on percussion over the left apex, and some loss of resonance over the right. The vesicular murmur over the rest of both lungs was nearly normal. On August 10, on making a hasty movement, he was suddenly seized with an acute pain in the left side, and with intense dyspnoea; his face was pale and anxious. Tympanitic resonance was found all over the left side, except for about four or five finger-breadths at the base, where there was marked dulness. Amphoric voice and breath-sounds with metallic tinkling were heard on auscultation, the thoracic vibrations were abolished, and succussion could be easily felt, even by the patient himself.

For some days the patient was alarmingly worse; the lower limbs became œdematous, and the evacuations were involuntary; he was thought to be on the point of death; but, after a struggle of several weeks, about the end of September, a favourable change took place, and from that moment he continued to mend daily. The dyspnoea diminished; strength, appetite, and sleep returned. The cough diminished; and the expectoration, before so abundant, almost completely disappeared. The physical signs at that time were dulness, and resistance to the



finger at the left base behind, exaggerated resonance in the other parts of the chest; the amphoric breath-sound and the metallic tinkling had disappeared, but succussion was still perceptible; the cavernous blowing and the subcrepitant *râles* had notably diminished. The heart remained displaced to the right. At the end of November he was able to return to his home in the country. Had he been able to pass the winter in a favourable climate, Dr. Hérard believes he might have been completely restored to health. Such cases as these are not rare or isolated ones; twenty such are collected in the thesis of Dr. Toussaint, from different authors.

How shall we explain the favourable influence of hydro-pneumothorax in these instances, and what is the mechanism by which a cure is effected? Is it possible to foresee in what cases such an issue may be hoped for, and in what cases a fatal event may be anticipated? Bearing in mind the anatomico-pathological conditions under which hydro-pneumothorax may occur, we may arrange the cases under three categories, founded on the state of the tuberculous lung.

In the first category, the lesions of the lung are in their earliest stage. The lung presents scattered granulations or small caseous nodules, in the midst of a tissue more or less hyperæmic, but in a condition in which any sudden compression might reduce it to a small volume. The occurrence of hydrothorax in such cases is not rare. All that is necessary in order to establish a communication between the bronchi and the serous cavity, is the softening of one of these small tuberculous masses on the surface of the lung. The first effect of such an accident is the compression and flattening of the lung by the escaped air and the liquid effusion which rapidly follows. Later on, false membranes envelop the organ in a sort of thick resistant shell, and maintain it reduced often to quite an insignificant volume against the vertebral column and groove. The lung, under these circumstances, is dense and carnified, and its functional activity is greatly diminished or completely suppressed; we can readily understand how, in such a case, the tuberculous evolution is checked, or sometimes altogether arrested.

In the second category the pulmonary lesions are much more advanced, and the phthisis has reached the second or third stage only, and this is the important point; these changes are limited to the upper lobe, the other lobes presenting merely granulations or small caseous masses, surrounded by more or less congested lung-tissue. As in the first category, the effect of hydro-pneumothorax, by compressing the lung, is to arrest its functions, and in diminishing the afflux of blood, to prevent the nutrition of the morbid products, and to stop, in short, the tuberculous process. At the same time another remarkable phenomenon takes place in the upper lobe; the walls of the cavities, yielding to compression, come together, become agglutinated, and sometimes even cicatrize; the pulmonary suppurations and excessive bronchial secretion diminish, and, as in the case cited, the expectoration is completely suppressed. This suppurative process being arrested, the consequent hectic fever, sweating, diarrhoea, etc., disappear also, and the patient passes from the most alarming state into one of sensible amelioration, and even of real cure. But to insure so favourable a result, it is necessary that the opposite lung should be and remain free from disease.

One of the cases cited by Dr. Toussaint gives anatomical demonstration of the cicatrization of cavities under such conditions. A young soldier presented all the signs of pulmonary tuberculosis in the third stage, gurgling, loud cavernous blowing, metallic bruits at the left apex, and a few subcrepitant *râles* at the right apex; abundant purulent expectoration, great emaciation, profuse sweatings, hectic fever, intractable dyspepsia. A few days after admission into hospital, a sudden access of suffocation, with pain in the left side, drew attention to the occurrence of hydro-pneumothorax. For some days the patient seemed on the

point of death, when slowly and unexpectedly he began to revive; the dyspnoea abated, the fever disappeared, and the appetite became good. The sweatings ceased, and the expectorations changed completely in character, and became scanty and glairy. This amelioration was maintained and augmented until some months afterwards, when an exudation into the right pleural cavity gradually increased, in spite of all that could be done, and the patient died. The necropsy showed the existence of double pleural effusion. The left lung was flattened and compressed upwards and backwards, and strongly bound down, so that it did not reach below the fourth rib; at its apex there were traces of a large cavity, the walls of which were in contact and had become agglutinated and cicatrized; it was represented by a gray, irregular band, about the size of the little finger. Two smaller bands indicated the existence of two other smaller cavities. These were surrounded by masses of hard cretaceous tubercle. • The right lung presented at its apex a few small masses of hard tubercle. These observations show the important part which the compressibility of the pulmonary tissue takes in the cure of such cases.

In the third category we find the whole of the lung invaded by tuberculous lesions (granulations and broncho-pneumonia); the organ is hard, compact, resistant, irreducible by compression; in such cases, hydro-pneumothorax can only increase the peril which already exists.

It is incorrect, then, to regard the occurrence of hydro-pneumothorax in the course of phthisis as always fatal; on the contrary, in certain well characterized cases, it may be of favourable import.—*London Med. Record*, Aug. 15, 1881.

#### *Heart Symptoms of Chorea.*

Dr. OCTAVIUS STURGES, Physician to the Westminster Hospital, London, publishes in *Brain*, July, 1881, an interesting paper, in which he shows that in chorea the heart is apt to sympathize with the voluntary muscles at all ages up to the adult period, this sympathy being shown as well by dynamic murmur as by accelerated action, unevenness of rhythm, and, not seldom, the excited impulse common in hysteria, the particular manner of response being dependent upon the age of the patient. The hypothesis is that these several modes of heart affection correspond with as many modifications of chorea, which are exhibited not in the heart only, but in the voluntary muscles as well; these several regions sharing jointly, each in its own degree and after its own manner, in a disorder the area of whose influence is coextensive with that of ordinary emotional disturbance; and particularly that in all such variations the motor element of the affection is represented mainly by irregularity and unevenness of cardiac rhythm, the emotion element by acceleration, and the paresis element by dynamic murmur.

#### *Treatment of Cardiac Diseases.*

According to Prof. RENZI, bromide of potassium diminishes the anxiety of patients with heart disease, and renders their respiration easier. Under its influence sleep is more tranquil, easier, and lasts longer. The number of cardiac pulsations and respiratory movements diminishes; the cough, however, seems to be aggravated by this remedy. Iodide of potassium succeeds better, particularly in ameliorating the respiratory symptoms and in overcoming dyspnoea. Hydrate of chloral serves to overcome the insomnia in cases of heart disease, but in general it does not diminish dyspnoea of cardiac origin. It tends, moreover, to increase cerebral torpor and somnolence, and when combined with iodide of potassium it tends to produce a serious and persistent somnolence.—*Journ. de Méd. de Paris*, Sept. 10, 1881.

*A Case of Prolonged Intestinal Obstruction.*

Dr. F. W. DRAPER, of Boston, reports the following case. The patient was a man, aged thirty-three, of sedentary habits. For six years, dating from an attack of "inflammation of the bowels," he had been subject to recurrent attacks of obstinate constipation, in one instance lasting ten days, and accumulation of flatus. On June 28, 1880, after indulgence in iced water, he was attacked with slight vomiting, distension of the abdomen, and moderate pain. The matter vomited was of a bilious character. The abdomen was uniformly distended and tympanitic, without tenderness, and careful palpation and percussion detected no region presenting exceptional signs. After the second day vomiting ceased spontaneously; there was obstinate constipation, which lasted for twenty-one days, in spite of the employment of laxatives, turpentine stupes to the abdomen, and stimulating enemata. Sulphate of magnesia was once employed, but increased the patient's misery; belladonna was employed until its physiological effects gave warning to desist. The intestines were four times aspirated, and large quantities of gas removed; large enemata of warm water were also employed without effect. On the twenty-first day flatus escaped from the anus, followed by scybalous faeces, and on the next morning a normal stool was voided; and for nine days, though weak, the patient appeared to be in good health and spirits. On the ninth his symptoms returned, but with increased severity, and death occurred seventy-two hours after the onset of the attack.

An autopsy was made thirty-two hours after death: The abdominal walls were distended to the utmost; when the usual longitudinal section was made, the intestines escaped through the incision in huge convolutions. The entire length of the small intestine and the ascending and transverse portions of the colon were fully loaded with mingled liquid and gaseous contents. The peritoneum was everywhere injected, and the vessels of the omentum were especially engorged. In the dependent portions of the abdomen there was an effusion of dark-red, thin fluid, amounting, by estimate, to eight fluidounces. The colour of the small intestine was a dark purple; its diameter was that of a large orange; its contents consisted of dark-brown, fetid liquid feces. The distended large intestine was of a pale colour; its diameter was six inches and its contents semi-solid faeces. Search was made for the points of puncture by the aspirator needle, but no remnant of that operation was found. There was no agglutination of the intestinal folds or other indication of peritonitis, beyond the intense general hyperæmia and the fluid exudation.

At the junction of the transverse and descending portions of the colon (splenic flexure) there was a cicatricial constriction. In the concavity of this flexure the three coats of the intestine were drawn into a firm, dense mass of tissue, the puckered folds of which contraction had reduced the calibre of the gut so as scarcely to admit the tip of the little finger. Above this stenosis the intestine was dilated into a great bag or reservoir filled with semi-solid faeces; below it the gut was contracted to its normal dimensions. Seen from within the intestine showed nothing remarkable at the constriction beyond the puckering of the mucous lining; on the outside (peritoneal surface), however, the stenosis was marked by a roughened, elevated ulceration of the size of a silver quarter-dollar. A fold of the jejunum was attached by loose adhesion of its free border to this ulcer, and its peritoneal coat presented a corresponding, though more superficial, loss of tissue. The situation and relations of the constriction were such that it could easily be plugged by a fecal mass of slight consistency.

Upon the surface of the large intestine, just above the ulceration, was a minute perforation which hardly admitted the head of a probe, and through this small

opening enough faecal matter had probably escaped to develop the peritonitis which destroyed the patient.—*Boston Med. and Surg. Journ.*, Sept. 22, 1881.

#### *Amyloid Degeneration of the Kidney.*

In the twenty-eighth volume of the *Deutsches Archiv für Klinische Medizin*, WAGNER publishes an interesting paper upon amyloid degeneration of the kidney. In regard to etiology, out of 265 cases, Wagner found that 136 accompanied phthisis; 56 occurred along with disease of bone (non-syphilitic); 36 accompanied syphilis; 30 were found in connection with certain rarer diseases; and with regard to 7 no probable mode of origin was apparent. The more interesting portion of this paper is that in which the author deals with the symptomatology of waxy kidney. In comparatively few of these cases were the symptoms observed during life so closely as to be of service; only twenty-four appear to have been available, and of these the majority were only under observation for very limited periods; the longest apparently only during a few months. Studying these cases in detail, Wagner separates them into four groups: 1. Slight amyloid degeneration of the cortical or the medullary substance, or of both, without alteration of the epithelium or of the stroma; this variety was hardly observed clinically; 2. Greater or less waxy degeneration of the cortical or medullary portion, with a varying amount of fatty degeneration of the epithelium, without changes in the stroma; 3. The same waxy degeneration, along with fresh interstitial changes; 4. Waxy-contracted kidney. In regard to this last-named condition, Wagner adopts the view held by Cohnheim, Bartels, Rokitsansky, etc., viz., that the waxy change occurs in the kidney after it has become cirrhotic. With regard to the condition of the urine in these cases, Wagner found that it was almost invariably diminished in quantity, and this without diarrhoea, perspiration, or increasing œdema being present. In a few cases the urine was of normal quantity, and only in two does it appear to have exceeded that amount. The quantity of albumen was in thirteen cases of the fifteen belonging to the first two groups, in medium or in great amount ( $\frac{1}{4}$ ,  $\frac{1}{2}$ ,  $\frac{3}{4}$ , or even 1 volume); but in two cases it was at the commencement small in quantity, afterwards increasing. The tube-casts varied much in number from one day to another. They were for the most part of medium diameter, and usually hyaline, sometimes fatty. Wagner believes that œdema is more rarely met with in such cases than is generally supposed. In conclusion he observes that, while waxy kidney may often be diagnosed with care, there are many cases in which such a diagnosis cannot be arrived at with any certainty.—*Lond. Med. Record*, July 15, 1881.

#### *The Pathogenesis of Albuminuria.*

The physiological experiments of Gull and Stokvis, of raising the arterial pressure in the kidneys by ligaturing arteries or compressing the abdominal aorta, showed that high pressure alone would not determine albuminuria. This may seem at variance with the clinical fact of the presence of albuminuria in interstitial nephritis with hypertrophied heart; but M. CHARCOT, in a recent lecture (*Le Progrès Médical*, 1881, No. 19), explains this by the fact that some of the glomeruli are more or less profoundly altered, in consequence of which both the pressure and the rapidity of the blood-current are diminished. [It is obvious that, if the changes in the glomeruli are to be called in question at all, M. Charcot is departing from the mechanical theory he has propounded.—*Rep.*] On the other hand, Overbeck's experiment of reducing the blood-pressure by introducing a sound into the right ventricle, carrying a small bladder which can be inflated at will, is always followed by albuminuria. Here the condition of things is dimi-

nution of pressure and rapidity. Such a condition is met with clinically in valvular disease of the heart, chronic disease of the myocardium, and some pulmonary diseases. In cases of mitral insufficiency, Rosenstein observed that the albumen disappeared as the urine increased, and returned as it fell in quantity. The cardiac kidney is not the cause of the albuminuria of heart disease, but the consequence of the state of things which causes the albuminuria. The albuminuria of Asiatic cholera and other acute diseases is also explained by this theory. The anatomical theory which refers albuminuria to a lesion of the renal epithelium, supported of late by Lécorché, is opposed by the experiments which show that albumen is excreted by the glomeruli, and that its presence in the convoluted tubules is a pathological occurrence, and by the fact that the epithelium may be profoundly altered or destroyed, as in granular kidney or in phosphorus poisoning, without albuminuria.—*Lond. Med. Record*, July 15, 1881.

—

*Simultaneous Existence of Albumen and Sugar in the Urine.*

In a paper read before the Berlin Medical Society, and published in the *Deutsche Med. Woch.*, No. 24, 1881, Professor FRERICHS says that the coexistence of albumen and sugar in the urine occurs under various conditions, which must be carefully discriminated. Three forms of this anomalous process of secretion must be distinguished: one occurring in glycosuria, another in diabetes mellitus, and a third in chyluria. In glycosuria—by which Frerichs denotes cases in which small quantities of sugar are discharged within a longer or shorter time without having any obvious influence on nutrition—albuminuria is frequently met with. Of thirty cases observed and collated by Frerichs, albumen was found in fourteen, that is, in nearly one-half. They were partly acute, partly chronic, diseases of the brain. Three times albumen and sugar were observed in the urine of patients affected with cerebral aneurism, which had burst into the ventricles; four times in extensive apoplexies; twice in purulent cerebral meningitis; and once in chronic tuberculous basilar meningitis. In all these cases the ventricle was found to contain blood, with the exception of one case, in which the apoplectic focus extended to part of the cerebellum and to the cerebral peduncle. The chronic form of the disease lasted for years, and there was no opportunity of making a *post-mortem* examination. In these cases Frerichs three times observed albuminuria, and also, in the further progress of the disease, simultaneous paresis of the facial nerve. The sugar in these cases amounted to from  $1\frac{1}{2}$  to 2 per cent., while the quantity of albumen varied. Sometimes the sugar disappeared, and then the albumen, until in some cases there was a retrogression of the renal secretion. At the same time polyuria existed; in other cases it was absent.

In all these cases of glycosuria, Frerichs thinks the secretion of albumen and sugar to have the same source, namely, a lesion of the fourth ventricle, since, when the injury is situated higher, there is secretion, not only of sugar, but of albumen also. In such cases, the prognosis must depend upon the nature of the cerebral lesion. Severe apoplexy, with lesion of the lateral ventricle, usually proves fatal in a few days; so also does cerebral aneurism. The course is more favourable in meningitis, in which recovery occurs in some circumstances. Frerichs reports two cases, in which the secretion of albumen and sugar ceased, and all symptoms disappeared.

In the chronic form, the prognosis must be cautiously made, for this mostly affects persons whose health is impaired by mental excitement or overwork, who complain of headache and indisposition, and are unable to attend to their daily avocations. Apoplectic injuries are associated with remarkable frequency with



glycosuria, a circumstance that points to an effusion of blood into the fourth ventricle.

In the same manner may be explained the coexistence of albumen and sugar in cases of poisoning, especially by carbonic oxide gas. In 12 out of 17 cases there was sugar; in three there were albumen and sugar; and in one only albumen, together with cylinders.

In diabetes mellitus, the condition is altogether different. It is assumed by many that diabetes mellitus will lead to nephritis. Griesinger, in 64 *post-mortem* examinations of cases of diabetes, found 32, that is, one-half, complicated with nephritis. Seegen found nephritis in 20 of 30 cases in the Vienna *post-mortem* house; while Dickinson found 25 of 27 cases in the St. George's Hospital to have nephritis. With that, however, Frerichs's experience does not agree. In true diabetes, albuminuria is not frequent, nephritis rare, and occurs only in certain conditions. Out of 316 cases of diabetes mellitus, which Frerichs partly observed for ten, twelve, and even fifteen and sixteen years, there were only 16 cases of nephritis (5 per cent.). Cases in which small quantities of albumen were occasionally found in the urine were never pure and simple cases, since it could be proved, at least in most of them, that other processes were involved, which of themselves could produce nephritis. Of 16 cases, 6 were complicated with arterial sclerosis; in 3 others there was pulmonary phthisis; in 2 cystitis; and in 2 arthritis with renal calculi. Therefore, only three cases remain in which no co-operating cause could be suggested. Thus it becomes evident that the general belief that the increased action of the kidneys in diabetes mellitus leads by fluxion to albuminuria and nephritis is erroneous, notwithstanding that it has numerous followers. According to Frerichs's *post-mortem* examinations, which comprise more than 50 cases, true nephritis is hardly ever met with in diabetes; and where it did occur, it was of a parenchymatous nature, moderate hyperæmia of the renal substance, or there was shrivelling of the kidney. It has been believed that the changes of the epithelium of the gland, now and then occurring in diabetes, and lately described by Ebstein, might contribute towards the production of albuminuria. These changes in the epithelium have been searched for in the clinic, and have been found where there was no albumen in the urine; so that Frerichs feels rather disinclined to go as far as Ebstein does in drawing conclusions. All cases which were complicated by pulmonary phthisis proved fatal, while in all cases in which albuminuria was produced by additional cystitis, nephritis, or arthritis, there was a favourable course. An old gentleman, aged 79, who was attacked with pneumonia while in Carlsbad, and was afterwards lying ill in Nice, suffered from albuminuria, on which cystitis supervened. Gradually the sugar disappeared, and albuminuria, with cylinders, set in. Then the albumen disappeared, and at present he may be regarded as cured.

There is yet another condition in which sugar, albumen, and fat occur together—chyluria. It remains, however, an open question how the sugar is transmitted into the urine along with the albuminates. Whether, as some believe, there is a damming back into the lymphatic vessels or a transition into the urine, consecutive upon chylolysis, has not yet been determined by anatomical observation. One thing only has been established by Brieger—that fat disappears from the urine, if it be withdrawn from the patient's food, and that it reappears in the urine as soon as it again becomes part of the food. It is much to be regretted that the experiment was not made with regard to sugar.—*London Med. Record*, August 15, 1881.

### Urinary Analysis.

A new method for the quantitative analysis of the extractive matters of urine has been worked out by MM. CHAVANE and RICHET, who claim for it the advantage of much greater simplicity than that of the methods at present in use. A solution of biniodide of mercury and iodide of potassium, to which potash has been added, is blackened by urine, and oxide of mercury is precipitated. The reaction takes place even in the cold, but is facilitated by heat. Neither the urea, chlorides, phosphates, nor sulphates are precipitated, but uric acid causes a white deposit from which heat causes oxide of mercury to separate. Alkaloids, formates, and acetates have no influence. If the reaction of the mixture is acid the precipitate does not form, a soluble salt of mercury being produced, but the alkalinity of the test solution is usually sufficient to insure the reaction. Salts of ammonia, moreover, mask the reaction by dissolving the oxide of mercury. Chloral, aldehyde, sugar, and all readily oxidizable substances also precipitate the oxide of mercury. Normal urine contains substances which have the same action, viz., nitrogenous substances soluble in ether and in alcohol. Hence this method gives a simple means of ascertaining the amount of colouring matter, extractive matter, and nitrogenous substances other than urea. The exact mode of proceeding is as follows: The composition of the solution is 10 grammes of biniodide of mercury, 20 of iodide of potassium, 50 of caustic potash, and 930 of distilled water. One burette is filled with the solution, another with urine. Fifteen drops of each are allowed to fall into a porcelain capsule, and it is heated and urine added until the mercury is proved to be precipitated by the absence of the reaction given by an alkaline solution of tin, with a small quantity taken up by a capillary tube. A litre of normal urine precipitates about five grammes of mercury.—*Lancet*, Sept. 24, 1881.

## Surgery.

### Antiseptic Treatment of Abscess.

Dr. LUCAS-CHAMPIONNIÈRE recommends the following procedure: Before opening an abscess, in whatever region it may be placed, we should carefully wash the skin, especially if it has been covered by a poultice, with a strong carbolic acid solution (crystals 50 parts, glycerine 50 to 75 parts, and water 1000 parts). The bistoury should also be dipped in the solution. The contents of the abscess are to be discharged, and some of the above solution injected, care being taken that the injected liquid has a free issue. The end of a caoutchouc tube is introduced into the wound, having a thread attached to it to facilitate its removal, and it is then covered by a thick layer of charpie, impregnated with a solution of carbolic acid 25 parts, glycerine 25 parts, and water 1000 parts. Finally, over all is laid a layer of gummed silk. At the end of twenty-four hours the tube is removed in order that it may be cleansed and shortened, when it is again covered with the charpie moistened with the weaker solution. Under this treatment the amount of suppuration is diminished, the redness of the wound becomes insignificant, and the cicatrices which result are much less apparent. Dr. Lucas recommends this procedure especially in abscess of the breast.—*Med. Times and Gazette*, October 8, 1881, from *Union Médicale*, September 15.

### Treatment of Malignant Lymphosarcoma with Arsenic.

Dr. F. THOLEN (*Arch. für Klin. Chir.*, Band xvii.) reports four cases of lymphosarcoma, in three of which the course of the disease was greatly influenced

by arsenic, while in the fourth case the disease had already advanced too far towards a fatal result. The first case was that of a man, aged 47, who suffered from an infiltrated epithelioma in the right angle of his lower jaw, of three months' duration. It could not be operated upon, because of the extent and infiltration of the neighbouring lymphatic glands, and was therefore treated with arsenic. From May 26 until the end of December, 1872, the patient took Fowler's solution; partly internally, from five drops a day to fifteen drops twice a day, and partly in the form of parenchymatous injections into the tumour, ten drops each time, amounting altogether to 748 drops internally and 76 injections. This treatment resulted in the complete disappearance of the tumour. The patient was discharged on January 15, 1873, and on returning in April, 1873, and in February, 1874, he showed no sign of recurrence. Examination of a small particle taken from the margin of the ulcerating tumour indicated a lamellar epithelioma, yet the appearance of the disease, as well as the favourable action of the arsenic, rather suggested an infiltrated malignant lymphosarcoma. The author, therefore, thinks it probable that epidermic cells from the adjacent healthy epidermis might have been present when the examination was made. From these cases, although few in number, Tholen is led to ascribe to arsenic a positive salutary action against cancerous ulcers, especially lymphosarcomata, which, in fact, are nearest to inflammatory neoplasms, and are quickly dispersed by other agents as well. A mischievous result has been several times observed by Billroth to follow this rapid breaking up, when iodide of potassium was energetically administered. —*London Med. Record*, August 15, 1881.

#### *Treatment of Burns by Bicarbonate of Soda.*

J. TROIZKY adds his testimony to that already published as to the value of solution of bicarbonate of soda as a dressing for burns (*Wratsch.*, 1881, No. 4; and *St. Petersburg Med. Woch.*, 1881, No. 19). He says that during the previous year he noticed twenty-five cases of burns, mostly of a severe nature. Sixteen of them were received in a fire in a village, during a strong wind, when the inhabitants, in order to save their property, were obliged to work in the flames. In all these twenty-five cases bicarbonate of soda was exclusively applied. The result of this treatment was so favourable, that the author considers himself justified in pronouncing this remedy the best and most efficient in burns of all kinds and degrees. Even in extensive burns of the second and third degrees, the pain was soon alleviated by the application of compresses soaked in a solution of bicarbonate of soda; and the wounds soon healed, leaving but few scars, and no impairment of the functions of the affected parts. No evil results from this extensive application of bicarbonate of soda, which might suggest the reception of carbonic acid into the blood, were noticed. Of the ten cases of which a detailed account is given by the author, one rather serious case may be related, in which the burns had spread over half the body. The whole face was stripped of epidermis, the hair was singed off, and the front of the neck, chest, and abdomen, and the dorsum of the foot, presented burns of the second degree; burns of the third degree were also found on the right mammary gland and on the right forearm, all the muscles of which were exposed as if prepared by dissection. A solution of the bicarbonate of soda was applied to these burns with a most conspicuous success: the patient felt relieved, and a cure was effected in four weeks; but the healing of the burns on the right breast and on the forearm lasted two months. The scars which were left were insignificant, and the mobility of the fingers was nearly normal. As regards the application of bicarbonate of soda in burns, the author distinguishes three methods. 1. Powdered

bicarbonate of soda is strewn over the burned parts. 2. Linen rags, sprinkled with a solution of bicarbonate of soda (1 in 50) are laid on; as soon as these rags become dry, they are replaced by others or are moistened again in the solution. 3. Linen rags are applied in the same manner, but are kept constantly upon the burns, and moistened by pouring the solution on them. The first method suffices only for burns of the first degree. Change of the moistened rags is chiefly adapted for burns of the third degree, attended with much suppuration. In exchanging the dry rags, the pus which has accumulated underneath them must be carefully washed off, that it may not be received into the blood; and then a fresh rag soaked with the solution must be placed upon the clean granulating surface. The third method is applied solely in burns of the second degree. Changing the compresses would in these cases only irritate the exposed surface, and, by causing a more copious suppuration, delay the healing process. The beneficent effect upon burns of the solution of bicarbonate of soda the author considers to be due to the anæsthetic, antiseptic, and disinfecting property, which the bicarbonate owes to the ready disengagement of carbonic acid from it. Herr Troizki has also made experiments with other antiseptic and disinfectant agents, but has come to the conclusion that these are inferior to the bicarbonate of soda in their effect upon burns. Besides, the application of some of these, as for instance of carbolic acid upon large surfaces, would be dangerous; on small surfaces they very thoroughly disinfect the secretions of wounds; but these disperse more slowly than by the use of soda, and the anæsthesia, so desirable for the patient, is not so complete as when soda is applied.—*London Med. Record*, Aug. 15, 1881.

—

*Case of Coin in Air-passages treated by Inversion of the Patient.*

Mr. JOSEPH BELL, Surgeon to the Royal Infirmary, Edinburgh, reports (*Edinburgh Med. Journ.*, June, 1881) the case of a waiter who amused himself by throwing into the air a sixpenny-piece just received from a traveller. He intended to catch it in his mouth, but the coin disappeared. Fancying it had passed into his œsophagus, he came to the Infirmary, where he was seen by the house-surgeon, Mr. Ross, who failed to find any evidence of its presence in either œsophagus or stomach, for the examination of the throat caused vomiting, and the coin was not found. No laryngeal or pulmonary symptoms were present, though patient pointed to his left chest as being painful. A laryngoscopic examination, however, satisfied Mr. Ross and Dr. Maxwell that the coin was lying edgeways in the left bronchus, just at the bifurcation. There being no symptoms, he was left quiet, all preparations being made for tracheotomy if necessary, and Mr. Ross slept in the side-room of the ward.

Next day Mr. Bell carefully examined chest and air-passages, with negative results; immediately after this, however, when he was in the act of operating on a sarcoma in the theatre, the patient was reported to be choking. Mr. Ross found him in a paroxysm of coughing, and when it passed off he described a feeling of fluttering or flapping in his windpipe. Dr. Wyllie and Mr. Bell then made a most careful laryngoscopic examination, and though they saw the tracheal rings most clearly, no foreign body could be made out.

The history and the symptoms of the one attack were to Mr. Bell's mind so convincing that the coin was in the air-passages, that, with the approval of his colleague, Mr. Duncan, he at once prepared to perform tracheotomy in the event of the failure of inversion. So, having obtained the patient's leave, and all things being ready, he was held by the heels by two dressers standing on the operating-table, and a smart blow was struck on his back opposite the left bronchus, while he was instructed to keep his mouth open and to breathe freely. The

sixpence at once fell into his mouth; a pretty smart cough coincided with its passing the glottis.

Cases of this kind are comparatively rare, and this one is an encouragement to attempt the inversion method. The surgeon must, however, be prepared to perform tracheotomy at once if the coin sticks in larynx and excites spasm, or lodges flatwise in upper part of trachea, and thus prevents ingress of air.

#### *Gastrotomy in Stricture of the Œsophagus.*

A paper by Dr. T. F. PREWITT on gastrotomy in stricture of the œsophagus will be found in the *St. Louis Cour. of Med.* It is accompanied by a table of fifty-nine cases. Dr. Prewitt considers the great difference in mortality from the operation in cases of this class, when compared with the same operation for the removal of foreign bodies, to be due to the exhausted condition of the subject. It is his belief that it can be greatly lessened if the operation be performed early. In a large percentage of fatal cases, even though union had taken place, no peritonitis existed, and death was due to exhaustion. Gastric fistula is not incompatible with long life. Of the forty-nine cases tabulated, forty were malignant, twelve cicatricial, three syphilitic, and in four the nature of the stricture was not given, but was almost certainly malignant. Of these cases, operated upon for malignant disease, the patients lived from fourteen days to six months, and one patient is still living comfortably. In the cicatricial variety six recovered, as also one where the stricture was of syphilitic origin. In but seven of the whole number is peritonitis stated to have existed. Exhaustion alone is assigned as the cause of death in the large proportion of cases.—*London Med. Record*, July 15, 1881.

#### *Perineal Calculi.*

Dr. C. MAZZONI, Professor of Clinical Surgery at Rome, read a paper on this subject at the London Congress which contained the following conclusions:—

1. Perineal calculi may originate in the bladder.
2. They may be formed in perineal fistulæ communicating either with the bladder or ureter.
3. They may be contained in a cyst formed by the walls of the ureter.
4. They may be found in the scrotum, and have no connection at all with the ureter.

*Treatment.*—In such conditions as are included under headings 1, 2, and 4, extraction of the stone results in a cure of the fistula. For those of class 3 it is imperative to make a permanent perineal opening into the urethra.

### **Midwifery and Gynæcology.**

#### *Time of Ligation of the Umbilical Cord.*

Dr. J. G. SINCLAIR COGHILL, in his address in Obstetric Medicine, before the British Medical Association, called attention to an extremely interesting and valuable communication with reference to the time and mode of separating the fœtus and umbilical cord which has been made by Ribemont, in a recent number of *Les Archives de Tocologie*, and which shows satisfactorily the great influence of the "thoracic aspiration" of the fœtus on the umbilical circulation before its ligation. This was first pointed out by Budin; but is denied, among others, by Schücking. Determined by the manometer, it was found that—1. Tardy ligation



of the cord benefits the child by increasing the quantity of blood which is required for the establishment of the third circulation, that is, the fetal pulmonary. 2. The immediate ligation of the cord deprives the infant of a quantity of blood, larger or smaller in proportion to the time of ligation; and it especially deprives it of necessary blood if the ligation has been applied before the child has breathed. 3. The early ligation of the cord thus compels the abstraction of the blood necessary to establish the pulmonary circulation from the general circulation. The result is a diminution of the arterial tension equal to one-third of the initial tension. 4. The cause of the penetration of the blood into the pulmonary circulatory system of the child is the "thoracic aspiration." This is proved by the constant superiority of the pressure of the blood in the umbilical arteries to that in the umbilical vein. Again, the thoracic respiration is observed to produce considerable oscillations in the tension of the arterial and venous blood. The uterine contractions are utterly insufficient to force any blood along the umbilical vein when the arterial pulsations of the cord have ceased. 5. Thoracic aspiration causes the *sufficient* and *necessary* amount of blood to enter the pulmonary vessels; *sufficient* because, under these circumstances, the tension in the arterial system does not fall; *necessary* because the arterial tension in the umbilical cord of a newly-born child is never seen to rise after tardy ligation of the cord. Professor W. T. Lusk, of New York, in corroborating Ribemont's views, says that, in children born pale and anæmic, and suffering from syncope, late ligation of the cord furnishes an invaluable means of restoring the equilibrium of the fetal circulation.—*British Medical Journal*, Aug. 20, 1881.

#### *Membranous Dysmenorrhœa in relation to Normal Menstruation.*

In a paper read at the Académie de Médecine, Dr. SINGRY stated that it resulted from the observations which he had made on a great number of women, that, in the physiological condition, the uterine mucous membrane is not, as is generally taught, eliminated in menstruation; but, under certain pathological conditions, the mucous membrane of the body of the uterus exfoliates, and is expelled at the catamenial period. This phenomenon, designated as "membranous dysmenorrhœa," is usually accompanied by severe pain and loss of blood. It does not constitute a special disease or morbid entity, and is observed under very variable conditions, with or without metritis. The exfoliation is the result of an exaggeration of the normal menstrual process, inducing a too great infiltration of the deeper layers of the mucous membrane and compression of the vessels of this region, and leading to elimination of the tissues situated above this layer. It will be thus understood that anything which prevents the blood issuing, as in the normal state, by the superficial vascular network of the mucous membrane, may be a cause of membranous dysmenorrhœa.—*Med. Times and Gaz.*, Sept. 24, 1881.

#### *Local Treatment of Chronic Metritis.*

Prof. AMANN, of Munich, read a paper on this subject, at the London Congress, of which the following is an abstract:—

Most cases of chronic metritis require local treatment for their cure. If the disease be limited to the mucous membrane of the cervical canal the treatment is comparatively simple, and cure can be affected by various harmless means. Greater difficulty is met with when chronic inflammation of the body or of the body and neck of the uterus calls for local treatment. For many years I have carefully tested the various means recommended during the last twenty years in the treatment of the affection in question, in hospital and private practice, in more than 3000 cases, and have come to the conviction that only one method

acts with certainty without being troublesome and *dangerous*. This is new only in the manner of its execution, and consists in the systematic cauterization of the cavity of the body, and eventually of the cervix of the uterus by means of an instrument like a sound, into a hollow in the upper end of which is fused lapis mitigatus. This can be employed as is self-evident, according to the behaviour of the endometrium, and the resisting power of the uterus in individual cases, at one time more frequently and thoroughly, at another more rarely and cautiously, and will have, according to the peculiarities of the special case, by itself alone, or in conjunction with other means (topical blood-letting, scraping off of growths of the endometrium), almost sure results. Only in a few cases of large tumours or severe bleeding granulations of the endometrium is the employment of the galvano-cautery or thermo-cautery necessary. The intra-uterine application of *lapis mitigatus* is, with the necessary caution, absolutely free from danger, and in a small number of cases only does it cause pain, which, however, is usually of short duration; sometimes also it gives rise to considerable but transient bleeding. Once only have I noticed, after a severe cauterization of the whole of the uterine cavity, dangerous metritis or peri-metritis, which, however, ended in a few weeks in complete recovery. Even slighter degrees of acute endometritis and acute metritis occur according to my experience in barely 2 per cent. of all the cases.

One of the chief advantages of the proceeding in question is, that it can be carried out without assistance in at least 95 per cent. of cases, and usually in five, exceptionally in ten minutes. Within this time, moreover, the cleansing of the uterine canal from mucus, a necessary preparation for cauterization, as well as the dilatation of the narrow canal by means of metallic dilators, can be accomplished. Further this procedure is applicable, when the condition is complicated by severe flexions and versions, either after straightening the organ immediately before cauterization, or if the canal be of normal width by cauterizing with the sound-like instrument forthwith. This is done without a speculum just as the sound is introduced. By reason of the rapidity of the cauterization, which is finished in three to five seconds, and because, by the revolving of the caustic holder, contact of the caustic with the individual parts of the endometrium is extraordinarily short, coating of the caustic with coagulated albumen is prevented. Immediately after cauterizing a large tampon of wadding soaked in a solution of tannin should be introduced into the vagina in order to prevent the caustic dissolved in the uterine cavity from escaping into and through the vagina.

#### *Total Extirpation of the Uterus for Cancer.*

This operation may be said at present to be on its trial; for surgeons are not yet agreed as to whether the prospect of benefit outweighs the risk. The statistical accounts that have as yet been published are incomplete, and therefore not quite in agreement. We want figures to show us, first, in what number of cases the operation itself proves fatal, and then, in how many of those who recover from the operation the disease returns. The statistics are not in agreement; first, because improvements are being made in the *technique* of the operation, and in estimating the probable future mortality of an operation, we must reject cases in which the operation was not done in the way which better knowledge has shown to be the safest, and also because some cases, published as cures, have afterwards relapsed. Bearing in mind these errors, the following statistics will be interesting: MIKULICZ (*Wiener Medizinische Wochenschrift*, 1880, No. 47) quotes from Ahlfeld a table of 66 cases, out of which 49 proved fatal, in 4 the operation could not be completed, and of the 13 who recovered, in 6 relapses occurred; of the remain-

ing 7, in some the period since the operation, at the time the figures were compiled, was too short to allow the occurrence of relapse to be considered improbable. Some later statistics are more favourable. KLEINWACHTER, writing at the beginning of the present year, collected 94 cases of operation, with 24 recoveries; but KALTENBACH, writing about the same time, out of 88 cases enumerates 30 as successes. These figures evidently want sifting. They all relate to cases in which the uterus has been extirpated by abdominal section. OLSHAUSEN has collected (*Berliner Klinische Wochenschrift*, No. 35, 1881) 41 cases in which the uterus was removed by the vaginal method; of these 29 recovered and 12 died. To these he adds 6 performed by himself, all of which, so far as the operation was concerned, were successful.—*Med. Times and Gaz.*, Sept. 24, 1881.

#### *The Ovary in Incipient Cystic Disease.*

DR. VINCENT D. HARRIS, Demonstrator of Physiology at St. Bartholomew's Hospital, and Mr. ALBERT DORAN, Surgeon to Out-Patients, Samaritan Free Hospital, having enjoyed exceptional opportunities for studying incipient cystic disease of the ovary, have, in a joint paper, contributed the results of their observations to the *Journal of Anatomy and Physiology*, July, 1881. In over one-eighth of the cases operated upon at the Samaritan Hospital Dr. Bantock and Mr. Knowsley Thornton find that, after the removal of a large ovarian cyst, the opposite ovary presents such distinct symptoms of early cystic disease, that they remove it, knowing that otherwise a second operation will almost assuredly be necessary at no very distant date. A large number of such ovaries have been collected, and their appearances, when fresh, carefully described; the history of the patients, and notes as to the absence or regularity of the menstrual functions, together with records of abortion, parturition, or sterility, have also been preserved. The ovaries, within a few hours after removal, were placed within a half per cent. solution of chromic acid mixed with equal parts of methylated spirit. When sufficiently hardened, sections were prepared from them by means of Williams's freezing microtome. These sections were stained in logwood solution, and then mounted. Lastly, all have been since examined minutely by Dr. Harris and Mr. Doran, and separate notes carefully compared.

The results of their researches are summarized as follows:—

1. There is strong evidence that multilocular cystic disease of the ovary may arise from two totally different ovarian elements.

2. Cysts may arise from partial dilatation and partial obstruction of enlarged and thickened bloodvessels, as Noeggerath maintains,<sup>1</sup> and we think, as he does, that many errors have arisen from imperfect knowledge of the appearance of bloodvessels in the ovarian stroma.

3. Cysts more frequently appear to originate in changes in those Graafian follicles that undergo involution without having ever ruptured; this includes a large majority of the follicles, when we remember the vast number found in the ovary at birth, and bear in mind that involution of many follicles takes place between birth and puberty.

4. The morbid changes which replace normal involution of the follicle are an active ingrowth from the stroma, and a long persistence of the cloudy tube-like bodies that represent the remains of the membrana propria of the follicle. These two processes sometimes proceed at an equal rate, sometimes irregularly.

5. When the relics of the membrana propria are slow to disappear, and the stroma slowly sends ingrowths amongst these relics, we find the cystic bodies

<sup>1</sup> "The Diseases of Bloodvessels of the Ovary in Relation to the Genesis of Ovarian Cysts." *Amer. Journ. of Obstetrics*, etc., vol. xlii., 1880.

described as seen in the fourth case, containing myxoma-cells partly, at least, connected with the outgrowths. Such delicate tissue, made up of these cells, must soon break down as the cyst becomes larger.

6. When the process of ingrowth of stroma into the follicle, during involution, is particularly active, the ingrowths interlace and rapidly form cystic spaces, including portions of the cloudy relics of the membrana propria, giving the appearances seen in the third case.

7. On the other hand, the stroma may show little or no tendency to develop ingrowths, but the relics of the membrana propria may break down very slowly, and end, not in simple effacement and incorporation with the stroma, but in slowly breaking down, as in the second case. This must necessarily end in the formation of a cyst full of a colloid or semi-fluid material, the completely broken down granulosa, etc.; previous theories on colloid degeneration of the stroma itself may be based on the overlooking of the intra-follicular origin of the colloid collections. In all cases of myxomatous or colloid changes, or simple rarefaction of tissue, we found full evidence that those changes were in degenerate follicles and never free in the stroma.

8. All these changes in the degenerating membrana propria and the tissue surrounding the follicle, begin as exaggerations of the normal process of involution, which is never a mere disintegration and degeneration of the follicle. Slavjansky, in the work we have quoted, gives accurate drawings of the normal process. Patenko traces these abnormal changes to sclerosis of the follicle-wall, under different conditions.<sup>1</sup>

9. These changes in the follicle do not appear due to inflammation; indeed, in old inflamed ovaries the atrophy of the follicles appears to be quicker and more complete than in healthy ovaries, so that no trace of them is left, excepting certain granular masses.

10. The manner in which the young cyst first becomes invested with its characteristic epithelium is obscure. We found that the "germinal epithelium" of the tunica albuginea and the cells of the granulosa of normal follicles never invaded the ovarian stroma, and the epithelial relics of the Wolffian tubes, that are usually found loose about the stroma, were never in close relation to any of the bodies we have described. In the case of the dilated vessels, the endothelium must be the most natural starting point of the epithelial lining of cysts derived from such vessels. Remembering the changes in the mucous membrane of a prolapsed vagina, the endothelium may, we can conceive, alter its character when the nature of the free space on which it borders has become completely altered. Should the theory of Cripps<sup>2</sup> be even partly correct, that nuclei in lymphoid or connective tissue may gather protoplasm around them till they form real epithelioid bodies lining spaces deeply situated in solid structures, we could readily account for the origin of the epithelial lining of any cyst. We do not profess to accept as positive that observer's theory, that the nuclei in the submucous tissue of the rectum are normally the parents of the columnar epithelium of the mucous membrane, although microscopical evidence tends to support it; his other opinion, expressed above, that such nuclei may become epithelioid, is far more probable. An area in any tissue, with such nuclei, breaks down—the nuclei bordering on the broken-down region must be placed at once in different circumstances to their normal state, deep in tissue and away from any free surface. We do not find such nuclei on free surfaces; we do find epithelium. In other words, we

<sup>1</sup> In Virchow's Archiv, May, 1881, he demonstrates the manner in which this process ends in the formation, not of cysts, but of "corpora fibrosa."

<sup>2</sup> Cancer of the Rectum. See particularly Pl. III. fig. 1, in that work.

see no difficulty in supposing that the epithelial lining of ovarian cysts is probably developed from nuclear elements in the bodies above described. It seems, on mere reasoning, less far-fetched to assert that this epithelial lining is derived from pre-existing epithelium. To this we reply that, on actual observation, detecting incipient cystic cavities and also healthy epithelium on the tunica albuginea, in the follicles, and, as embryonic relics in the stroma, often in one single section, we find no connection between the cavities and any of these three normal epithelial elements. We must not conclude that when the mucoid contents of the imperfectly involved follicles have broken down to their utmost, the cellular elements immediately adjacent to the resulting semi-fluid material, simply assume the epithelial type. Only as long as the source whence normal epithelium is renewed remains obscure, so long must this question remain unsettled.

#### *Remarks on Antiseptic Ovariectomy.*

MR. KNOWSLEY THORNTON made the following remarks to a number of the members of the International Medical Congress who assembled at the Samaritan Hospital, on Aug. 3d:—

This being, he said, the last of our regular operation days before the hospital is closed for its autumn cleaning, I think it is a good opportunity to make a statement as to the work done in my wards since the 1st of January in this year. I have performed ovariectomy twenty-eight times, and I have only lost one patient, a child of fifteen, from whom I removed a solid carcinoma of 7 lbs. weight, involving both ovaries, the cæcum, and sigmoid flexure, so that I had to carve the intestines out of the solid growth. She died of exhaustion in forty-eight hours. Every one of these operations was performed with the 2½ per cent. spray, and solutions of absolute phenol (pure carbolic acid), and in no case was drainage employed. All the wounds healed by first intention, and the dressings were changed for the first time as a rule from the ninth to the twelfth day, when all the sutures were removed. Nineteen of the patients recovered without fever (*i.e.*, the temperature never rose even during reaction beyond 101° F.); five had temperatures from 101.6° to 103.4° during a few hours, but required no special treatment; four had temperatures from 102.8° to 104.6°, and required the use of the ice-water cap during periods of from thirty-nine hours to sixty-eight hours. These nine cases were all of more than usual gravity, with large tumours, extensive adhesions, long exposure, and much sponging. Only two of the patients remained in the hospital twenty-eight days after operation: one a patient from whom I removed an extensively adherent tumour weighing 80 lbs.; and the other one of the cases requiring the ice-water cap; a case of ruptured colloid, which I kept in bed longer than usual. The average stay in hospital after operation of the other patients was twenty days and a half, the shortest seventeen days. There were no adhesions in fourteen cases, slight adhesions in one case, and extensive adhesions with other complications in thirteen cases. In nineteen of the cases only one ovary was removed; in the others both were diseased and removed. To these twenty-eight cases of my own, I may add four cases operated upon in my wards by my colleague, Mr. MEREDITH, in which strict Listerism without drainage was also employed; all recovered. So that we have thirty-two Listerian ovariectomies without drainage with only one death, and that a case of cancer. I have also performed eight other abdominal sections: A removal of uterine fibro-cyst, weighing 23 lbs., together with both ovaries; a removal of several sessile and pediculate fibroids; an enucleation of a uterine fibro-cyst weighing 10 lbs.; a complete hysterectomy, with removal of both ovaries; and three oöphorectomies, undertaken for various conditions. All the patients recovered with the exception of the hysterectomy, and she died of septicæmia. I had to open a



much enlarged uterine cavity, which contained putrid material, and I failed to correct the putrefaction. This was the only case in which it was impossible to perform a strictly aseptic operation, and it was the only one terminating fatally. The total result is forty abdominal sections, with two deaths—a mortality of only 5 per cent. I may add that in no case were there symptoms of carbolism, though the  $2\frac{1}{2}$  per cent. solutions were used with all necessary freedom, and some of the patients had albuminuria before the operation.—*Lancet*, Aug. 20, 1881.

### Medical Jurisprudence and Toxicology.

#### *Notes of a Case in which Chrysophanic Acid was Administered Internally by Accident.*

Mr. JOHN GLAISTER reports in the *Glasgow Med. Journ.* for October, 1881, a case in which the patient was given a powder containing, by accident, a three-grain dose of chrysophanic acid, having been in fairly good health before. Within two hours after she was seized with the symptoms of irritating poison—burning pain in stomach, vomiting, pain in bowels, diarrhoea, hæmaturia, and pain over bladder, with tenesmus. It would, therefore, seem clear that the symptoms followed the medicine—*post hoc propter hoc*. It was also very evident from the symptoms that considerable irritation was produced in the gastro-intestinal and urinary tracts, occurring in that order. Hence, Mr. Glaister infers that a dose of three grains acts as a strong irritant. The colour of the matter first vomited, and the urine, point to the chrysophanic acid, because it is well known to produce coloured stains on linen coming in contact with it when used in an ointment.

But what are we to say about the bladder condition? The prolonged retention of urine pointed to one of two things—paralysis of the bladder through inflammation or disinclination to initiate the act of micturition owing to the great pain felt during that time. That there was inflammation of the bladder there can be no doubt, the persistent pain over that region indicating as much; and that this inflammation had ended in ulceration Mr. Glaister is inclined to believe, although some may gainsay it. He feared that the catheterism had had something to do with its production, but it was after the lengthened use of the catheter, when the parts had become accustomed to it, that the blood made its appearance, consequently he inclines rather to the previous explanation.

Previous to this case Mr. Glaister was not acquainted with the internal use of chrysophanic acid, and it was only in looking into the literature of this drug that he fell upon the recorded observations of Dr. Ashburton Thompson in the *British Medical Journal* for 19th May, 1877, pp. 607-8, where he calls chrysarobin—the powder in which chrysophanic acid exists to the extent of 80 per cent.—“a new emetic purge.” The action of the powder in doses of six grains to children of twelve, according to this experimenter, produced the following effects:—“Vomiting is always the first sign of action. This is not attended by any depression at all comparable with that caused by tartar emetic or ipecacuanha. In the doses presently to be named, it has not caused any distressing retching; and in children, as well as in adults, the acts of vomiting varied between nine in three out of the number, and six in two out of the whole number. They were usually two or three; very often only one. The action on the bowels was much more variable—from none in a few cases to nine or ten in equally few cases; most often the range was between three and seven. There is no griping pain, but the nausea continues more or less markedly until the bowels recover. . . . If the dose be taken into a full stomach, that delays its action and determines it

to the bowels." Dr. Thompson concludes "that chrysarobin is, in a dose of twenty-five grains for adults, or of six or more grains for children, an emetic purge of which the action is unattended by any inconvenient symptoms." Then as to the action of chrysophanic acid, pure. The same observer says that its action "is similar to the action of chrysarobin, with this difference, that while in a suitable dose each will cause vomiting and purging, if the dose be too small, chrysarobin is most likely to purge only, while chrysophanic acid is most likely to cause vomiting only." . . . A dose of "from fifteen to twenty grains will always both vomit and purge the patient very freely, at the same time that it causes an inconvenient amount of either of those effects very rarely indeed. Farther, there is but little danger of inconvenience from too large a dose."

The dose of the drug, as recommended by Dr. Thompson, is from six grains upwards.

Wood, in his *Treatise on Therapeutics*, p. 437, speaking of the action of this acid, says that "according to Schlossberger, Bucheim, Meykow, and Auer, it is not purgative; but Schroff has found it to be so."

In Professor Charteris' cases, recorded in the *Lancet* lately, where comparatively large quantities of strong chrysophanic acid ointment were used externally, symptoms of gastro-intestinal irritation always asserted themselves in the early part of the treatment, but afterwards disappeared. It is therefore quite clear that this acid acts as a gastro-intestinal irritant; the question which then naturally suggests itself is, Can it be given internally in doses which would produce therapeutic action, and fall short of producing severe symptoms such as experienced by my patient? In answer to this, Dr. Thompson, after an extended series of observations in the use of the drug, says that such a dose can be given without producing severe results. This case of Mr. Glaister, however, shows that even such a small dose as three grains will produce effects which are unpleasant and very severe; and it also points to the fact that its effects are not always to be referred to the gastro-intestinal tract simply, but to the urinary tract also. The question has now arisen in the minds of some of those who are accustomed to the use of this drug externally in skin diseases, whether the same beneficial effect now obtained by such use could not be more speedily and comfortably got by its internal administration. This has arisen from the fact that, in some cases, where the acid ointment has been applied only to one side of the body, the other, perhaps equally as much affected, has been nearly as quickly cured; and this would point to the absorption of the drug into the body. Every one knows that it cures skin affections by the irritant action it produces where it is applied—by its altering the character of the inflammatory condition producing the chronic disease, and giving it, as it were, a more acute tone. But how does it act internally? It certainly acts as an irritant, but whether directly as the metallic irritant poisons, or indirectly as apomorphia, is a question not yet settled. The presence of food in the stomach prevents its early action, thereby pointing in the direction of the behaviour of an ordinary metallic irritant. Its mode of exhibition, too, influences the rapidity of its action; when given with an alkaline solution, it acts more quickly, things being equal, than it does undissolved, as in water or an acid medium. So that, given in a powder, as in the case of my patient, and into an empty stomach, it will, by reason of its being undissolved in the water used as a vehicle for taking it, and also because it would remain undissolved in the acid contents of the stomach, probably act as a direct irritant; when, however, it has been exhibited dissolved, as in an alkaline solution, or where it has passed from the stomach with food and only been dissolved by contact with the alkaline juice of the pancreas, and then absorbed, it will probably act as an indirect irritant.

In the light of the foregoing case, Mr. Glaister confesses, even in the face of the recorded observations of Dr. Thompson, that he would be very chary as to the dose he would administer internally. Probably an unpleasant introduction to the use of a drug regulates one's feelings as to its general behaviour, and possibly that may be so in my case: in any case, I would prefer to begin its administration internally in much smaller doses than are recommended by Dr. Thompson.

Possibly, again, the element of idiosyncrasy had something to do with its severe behaviour in the person of Mr. Glaister's patient; that much might be argued; but he doubts if any one would question the severity of the effects, which were carefully noted as they appeared, more with the view of being recorded as the eccentric behaviour of the eunymon, no suspicion having arisen in my mind at the time as to an error in the dispensing of the powder.

#### *A Case of Nicotine Poisoning.*

A case of poisoning, with sudden death, is reported by L. L. DORR, occurring in a cigar-maker 49 years of age, and apparently due to excessive smoking and prolonged presence in a room filled with tobacco smoke. At the autopsy, the description agreed with von Borek's description of the morbid state (*vide Ziemssen's Cyclopædia*), viz., "The heart is generally empty, and the blood in the vessels is of a dark-red colour. The liver, spleen, and kidneys are generally hyperæmic. Vascular engorgements of the brain and its membranes, and serous fluid in the ventricles of the brain, are mentioned in almost all published reports."—*New Remedies*, March, 1881.

#### *Iron as a Poison.*

ORFILA demonstrated that large doses of sulphate of iron will cause death with symptoms of collapse and insensibility, and, post mortem, congestion and ecchymoses in the gastro-intestinal mucous membrane, and remarkable black discoloration of the blood. Frank arrived at similar results, employing the citrate or bromhydrate. Recently MEYER and WILLIAMS have found similar effects from the subcutaneous injection of the potassio-tartrate of iron, slightly alkalinized. About seventy-five thousandths of a grain constitutes a poisonous dose for the frog, and about six-tenths of a grain for a rabbit. In the frog, after absorption, there are paralysis of movement and disappearance of muscular irritability, without affection of the heart. In rabbits a great acceleration of the respiratory movements is first produced, and at the end of a little time diarrhœa comes on, with muscular weakness, while paralysis of movement, dyspnea, and convulsions precede death. The lungs and heart are found healthy, the duodenum is swollen and hyperæmic, the mesenteric glands are engorged, and the liver, kidneys, and spleen are congested. Similar effects are produced in the cat by four or five tenths of a grain, and in the dog by rather more per pound of body weight. The toxic symptoms are accompanied by a remarkable diminution of blood-pressure, explicable by vascular paralysis. There is also observed the black tint of the blood signalized by Orfila, which is not, as might be imagined, due to the accumulation of carbonic acid in the blood, since analysis proves that this is really diminished.—*Lancet*, Sept. 24, 1881.

### Hygiene.

#### *Prevention of Scarlet Fever.*

Dr. DAVID PAGE, at the London Congress, made some interesting remarks on this subject, in which he said, at the outset, that those precautions which, in  
Vol. XXXIX.—44

respect of scarlet fever, prove successful for the protection of a neighbourhood or a household, must also be the real defences for the community which is immediately concerned, and therefore for other and more distant communities. All national or international preventive measures against the diffusion of such a disease as scarlet fever must, in their essence, be measures for effectual individual control. These measures ought to give practical effect to the questions:—

1. What ought to be done with the infected individual or patient?
2. How long may the patient continue to be a possible source of infection?
3. What is the latest period, after exposure to infection, at which the disorder will show itself in a person who has received the infection into his system?
4. How soon may a person who has been exposed to risks of infection be pronounced safe against attack?

Two main clinical facts are involved in these questions:—

1. The duration of the incubation period of scarlet fever.
2. The modes of exit from the system of the fever poison.

A knowledge of the incubation period essential in disposing for the time of a person, who, having been exposed to chances of infection, may have contracted the disorder. In this case an appreciation of the maximum period of latency required. Literature of subject vague and untrustworthy.

The author's observations coincide with those of the late Dr. Murchison, and may be summed up:—

1. The common duration of the incubation period in scarlet fever is from twenty-four to forty-eight hours.
2. The period is occasionally longer, lasting from three to five days.
3. In rare instances practically absent, the symptoms following quickly upon exposure to infection.

Based upon these conclusions, the author requires that a person who has been exposed to infection should, before being pronounced safe from its probable consequences, be kept under surveillance for a week, and only then, after change of clothes, and baths, be set at liberty.

Of even greater importance than this is the action which ought to be taken in regard to the fever patient himself. This action is rendered difficult by reason of diversity and inconsistency of what are commonly looked for as the diagnostic features of scarlet fever.

An estimate of the real value of desquamation of the cuticle as a trustworthy guide in preventive measures must be based upon knowledge of irregularity of process in relation to time, quantity of eruption, or intensity of attack. Error of regarding infective process at an end before cessation of desquamation and *for some considerable interval afterwards*. Importance of pathology of scarlet fever as regards the nature of the eruption and the tendency to albuminuria, as a guide to preventive action and treatment.

The author's experience points to the necessity for isolation of the scarlatinal patient for a period of not less than eight weeks, and he would state the rule of Nature to be isolation for this period as a minimum, and, in cases of protracted desquamation or of relapse, until entire cessation of the process *and for a fortnight afterwards*.

For the rest, official action should include:—

1. Due provision for the separation of the sick from the healthy in the way of hospitals and convalescent homes.
2. Notification of the occurrence of cases of infectious disease.
3. Continuous supervision of all scarlatinal convalescents.
4. Control of school attendance of children belonging to infected families.
5. Disinfection and purification of infected houses and things.

## MEDICAL NEWS.

---

### DEFECTIVE PLUMBING AND DRAINAGE.

Our publication in the present number of the excellent plan of plumbing and drainage, approved by the New York City Board of Health, will we trust direct attention to a subject which requires the most careful consideration, not only in this community, but also in every town and even in every village throughout our country. The people of America stood aghast, as they well might, before the devastation caused a few years since by yellow fever, in New Orleans and Memphis; and yet it is probable that twice as great a proportionate annual mortality occurs, in every closely built city, from typhoid fever, diphtheria, and allied diseases, resulting from sanitary faults in plumbing and drainage.

That these dangers to our health and our lives are real and imminent, is shown by a moment's glance at the record of facts brought to light by Dr. H. R. Storer in his valuable report of the workings of the Sanitary Protective Association of Newport, R. I., in which he relates that the inspecting engineer of the society had found soil pipes both of iron and clay cracked, that joints were discovered having no cement or solder whatever, while frequently they were merely plastered over, leaving spaces whence sewage escaped; other joints being found so imperfectly closed, that rootlets of trees had entered, and by their growth entirely plugged up the pipe, with the result of backward flow and leakage of sewage. Soil pipes were likewise met with where old fractures had been plugged with wood, which in time, of course, had rotted away, and allowed sewer-gas to escape. Direct communications were unearthed moreover, between house and cess-pool, with no intervening trap, detection being made through inspection alone, there being no perceptible odour, although such must have been the state of things for more than a year, in some instances.

Last but not least in the long list of faults actually revealed by inspection, in the houses of subscribers to the Newport Association, it is to be noticed, that cess-pools were constantly discovered in close proximity to wells, thus suggesting an examination of the well water, which was rarely found good in any portion of the city.

Among medical men of the present generation, there exists, it is to be hoped, little need of dwelling upon this unsavory subject, but we desire to urge on every physician whose eyes these remarks meet, the vital importance of impressing upon his circle of friends and patients, how dangerous



to health are these constantly occurring blunders of defective plumbing. Thousands of valued lives have been sacrificed by typhoid fever, through the ignorance of architects and builders in regard to the first principles of sanitary drainage; and tens of thousands of beloved children have died from diphtheria and other filth diseases, in consequence of culpable carelessness or deceit on the part of plumbers.

The philanthropic efforts of sanitarians would probably be much aided, if the public could be persuaded to understand, that the risk of contaminating the human organism with the poison of typhoid fever (and perhaps of other diseases) is twofold; that is to say, the typhoid germ may enter our bodies by way of drinking-water (into which it finds its way in consequence of faulty construction and arrangement of drain pipes, etc.); or it may gain access to our systems through the lungs, by our breathing-air (if that air becomes mingled with sewer- or cess-pool gas), as a result of leaky and defective pipes, traps, or reservoirs for sewage. Moreover, it must always be remembered that no plumber's work, however complete it may be at first, can be relied upon to remain perfect; and that therefore the only sewage and drain-pipe arrangement, which is worthy of any confidence, is one in which nothing is concealed, but all parts may be easily inspected and repaired, whenever and wherever the slightest defect is found to exist.

In his efforts to secure due attention in the community to this all-important subject of proper plumbing and drainage, the sanitarian is baffled by an incredulity as to the impending danger, based upon the circumstance that individuals were known to have drunk water contaminated with the soakage of a cess-pool, or breathed air rendered foul by the emanations of a leaky trap or drain pipe, *for years* without apparent injury. Hence it is furthermore very important to disseminate in the community a knowledge of the fact that air or water rendered impure by admixture with simple ordinary filth, may be taken into the economy for a long time without obvious ill effects, but if this filth becomes infected with the specific poison or *germ* of typhoid or diphtheria (as may happen at any time, by the passage into it of the excretions from a person affected with either of these maladies), it is changed at once into the virulent carrier of disease. No better illustration of this great truth could well be given than the remarkable instance recorded by Prof. Flint, in which a young man travelling by stage coach in Vermont, fell ill with typhoid, was left at the hotel of a wayside hamlet, and within a week infected with his malady (through his intestinal discharges) all the families on the tavern side of a creek, which divided the village, *except one* whose head had quarrelled with the landlord, and drank none of the water from his well. Doubtless in this case the tavern-keeper's well-water had been contaminated with sewage from the hotel for years, and it would seem without serious injury to those using it, yet directly after the simple filth it previously contained was impreg-

nated, *in addition*, with the self-propagating germ of typhoid fever, this hitherto comparatively innocent water was altered into a powerful and deadly poison, for those who drank it. So strong is this single item of evidence, in regard to the dangers arising from impure water, that could it alone, without any other testimony, be heard by every one, we might soon expect a vast diminution in the evils from defective plumbing and drainage throughout the civilized world.

—  
*Rules for Drainage and Plumbing.*—A system of rules to govern the plumbing and drainage of all buildings hereafter erected in New York has been adopted by the Health Board, in accordance with the power conferred upon it by the statute enacted by the State Legislature June 4, 1881. These rules were distributed to plumbers on October 11, and are as follows:—

1. All materials must be of good quality and free from defects; the work must be executed in a thorough and workmanlike manner.

2. The arrangement of soil- and waste-pipes must be as direct as possible.

The drain-, soil-, and waste-pipes, and the traps, should, if practicable, be exposed to view for ready inspection at all times, and for convenience in repairing. When placed within walls or partitions, they should be covered with wood-work, fastened with screws, so as to be readily removed. In no case should they be absolutely inaccessible.

It is recommended to place the soil and other vertical pipes in a special shaft, between or adjacent to the water-closet and the bath-room, and serving as a ventilating shaft for them. This shaft should be at least two and a half feet square. It should extend from the cellar through the roof, and should be covered by a louvered skylight. It should be accessible at every story, and should have a very open but strong grating at each floor to stand upon.

3. Every house or building must be separately and independently connected with the street-sewer by an iron pipe caulked with lead.

4. The house-drain must be of iron, with a fall of at least one-half an inch to the foot, if possible.

It should run along the cellar-wall, unless this is impracticable, in which case it should be laid in a trench, cut at a uniform grade, walled up on the sides, and provided with movable covers, with a hydraulic concrete base of four inches in thickness on which the pipe is to rest.

It should be laid in a straight line, if possible. All changes in direction must be made with curved pipes, and all connections with Y-branch-pipes and one-eighth bends.

It must be provided with a running trap placed at an accessible point near the front of the house. The trap must be furnished with a hand-hole for convenience in cleaning, the cover of which must be properly fitted and the joints made tight with some proper cement.

There should be an inlet for fresh air entering the drain just inside the trap of at least 4 inches in diameter, leading to the outer air and opening at any convenient place not too near a window.

No brick, sheet-metal, or earthenware flue shall be used as a sewer-ventilator, nor shall any chimney-flue be used for this purpose.

5. Every soil-pipe and waste-pipe must be of iron, and must extend at least two feet above the highest part of the roof or coping, of undiminished size, with a return bend or cowl. It must not open near a window nor an air-shaft ventilating living rooms.

Horizontal soil- and waste-pipes are prohibited.

There should be no traps on vertical soil-pipes or vertical waste-pipes.

6. All iron pipes must be sound, free from holes, and of a uniform thickness of not less than one-eighth of an inch for a diameter of two, three, or four inches, or five thirty-seconds of an inch for a diameter of five or six inches; and for large buildings the use of what is known as extra heavy soil-pipe is recommended, which weighs as follows:—

2 inches,	5½	pounds	per	lineal	foot.
3 "	9½	"	"	"	"
4 "	13	"	"	"	"
5 "	17	"	"	"	"
6 "	20	"	"	"	"
7 "	27	"	"	"	"
8 "	33½	"	"	"	"
10 "	45	"	"	"	"
12 "	54	"	"	"	"

Before they are connected they must be thoroughly coated inside and outside with coal-tar pitch, applied hot, or some other equivalent substance.

Iron pipes, before being connected with fixtures, should have openings stopped, and be filled with water and allowed to stand twenty-four hours for inspection.

7. All joints in the drain-pipes, soil-pipes, and waste-pipes must be so caulked with oakum and lead, or with cement made of iron filings and sal ammoniac, as to make them impermeable to gases.

All connections of lead with iron pipes should be made with a brass sleeve or ferrule, of the same size as the lead pipe, put in the hub of the branch of the iron pipe and caulked in with lead. The lead pipe should be attached to the ferrule by a wiped joint.

All connections of lead pipe should be by wiped joints.

8. Every sink, basin, wash-tray, bath, safe, and every tub or set of tubs, must be separately and effectively trapped. The traps must be placed as near the fixtures as practicable. All exit-pipes should be provided with strong metallic strainers.

9. Traps should be protected from siphonage by a special metallic air-pipe not less than one and one-half inch in diameter; if it supply air to a water-closet trap, not less than two inches in diameter, the size to increase with the number of water-closets.

These pipes should extend two feet above the highest part of the roof or coping, and, if independent of the upper end of the soil-pipe, this extension should not be less than four inches in diameter to avoid obstruction from frost. If, however, they are branched into a soil-pipe, it must be above the inlet from the highest fixture. They may be combined by branching together those which serve several traps. These air-pipes must always have a continuous slope, to avoid collecting water by condensation.

10. Every safe under a wash-basin, bath, urinal, water-closet, or other fixture must be drained by a special pipe not directly connected with any soil-pipe, waste-pipe, drain, or sewer, but discharging into an open sink, upon the cellar-floor, or outside the house.

11. No waste-pipe from a refrigerator shall be directly connected with the soil- or waste-pipe, or with the drain or sewer, or discharge into the soil, but it should discharge into an open sink. Such waste-pipes should be so arranged as to admit of frequent flushing, and should be as short as possible, and disconnected from the refrigerator.

12. All water-closets inside the house must be supplied with water from a special tank or cistern, the water of which is not used for any other purpose. The closets must never be supplied directly from the Croton supply pipes. A group of closets may be supplied from one tank, if on the same floor and contiguous.

The overflow-pipes from tanks should discharge into an open sink or into the bowl of the closet itself, not into the soil- or waste-pipe, nor into the drain or sewer. When the pressure of the Croton is not sufficient to supply these tanks, a pump must be provided.

13. Cisterns for drinking-water are objectionable; if indispensable, they must never be lined with lead, galvanized iron, or zinc. They should be constructed of iron or of wood, lined with tinned and planished copper. The overflow should be trapped, and should discharge into an open sink, never into any soil- or waste-pipe or water-closet trap, nor into the drain or sewer.

14. Rain-water leaders must never be used as soil-, waste-, or vent-pipes; nor shall any soil-, waste-, or vent-pipe be used as a leader.

When connected with the house-drain, the leaders should be trapped beneath the ground, with a deep seal, to avoid evaporation, and, if placed within the house, must be made of cast-iron, with leaden joints.

15. No steam-exhaust will be allowed to connect with any soil- or waste-pipe.

16. Cellar- and foundation-walls should be rendered impervious to dampness, by the use of asphaltum or coal-tar pitch in addition to hydraulic cement.

Subsoil drains should be provided whenever necessary.

17. Yards and areas should always be properly graded, cemented, flagged, or well paved, and drained by pipes discharging into the house-drain. These pipes should be effectively trapped.

18. No privy-vault, or cesspool for sewage, will be permitted in any part of the city when a sewer is accessible.

*Raid on Poor Milk.*—The New York Health Board is at present engaged in very energetic efforts to prevent the sale of inferior milk. The milk inspectors make careful examinations of the milk when it reaches the city, and pour into the street all that does not conform to the standards and tests established by the Board. 5000 quarts of poor milk were thus spilled during the last week in September. A large number of milk dealers have also been arraigned and fined for the sale of adulterated and inferior milk.

*The International Pharmacopœia.*—A project of no small importance—viz., construction of an International Pharmacopœia, which should include a common mode of preparing and prescribing the more important drugs—has been considerably advanced during the last month, as the result of the conference of the International Pharmaceutical Congress held in London. The following resolutions were unanimously passed at this Congress; and will, we believe, be practically acted upon:—

"1. The fifth International Pharmaceutical Congress, held in London, confirms the resolution passed at the previous Congresses, as to the utility of a Universal Pharmacopœia, but is of opinion that it is necessary at once to appoint a Commission, consisting of two delegates from each of the countries represented at this Congress, which should prepare within the shortest possible time a compilation in which the strength of all potent drugs and their preparation is equalized. 2. The Executive Committee of this Congress is requested to take the necessary steps that the resolution be speedily carried out. 3. The work, when ready, should be handed over by the delegates to their respective Governments or their

pharmaceutical committees. 4. It is desirable that the Committee suggest a uniform systematic Latin nomenclature for the Pharmacopœias of all countries. 5. It is desirable that the Committee take measures that an official Latin translation be made of the Pharmacopœias of different countries which are not now published in that language. 6. It is desirable that the Committee be put in possession of all the manuscripts, including the documents relating to the Universal Pharmacopœia, compiled by the labours of the Society of Pharmacists of Paris, presented at the fourth meeting of the International Congress of St. Petersburg by the Society of Pharmacists of Paris. 7. That the pharmaceutical societies of the respective countries be requested to nominate those members of the Commission not appointed by this Congress, and to fill up any vacancies which may arise from time to time."—*British Med. Journal*, Aug. 20, 1881.

The representatives of the United States on this Committee are Mr. J. M. Maisch, of Philadelphia, and another to be chosen.

*Health of New York.*—The city's mortality for September somewhat exceeded that of August. For the week ending Sept. 3, there were 864 deaths. For the weeks ending Sept. 10, 17, and 24, there were respectively 866, 688, and 705 deaths. The total number for September thus amounted to upwards of 3100, while that for August was about 3000. The increased mortality may in a measure be accounted for by the fact that unseasonable, and, at times, excessive heat prevailed during almost the entire month, and gave rise to many fatal cases of infantile gastro-enteritis. The records of the Bureau of Vital Statistics show that 28,567 deaths have occurred during the past nine months. This number exceeds that for the corresponding period of 1880 by 4715. The total number for 1880 was 31,866, while the present indications are that this year's mortality will reach nearly 38,000. This death record, if actually reached, would be the highest known since the establishment of the Bureau, the next highest being that of 1872, which was 32,647.

The Board of Health cherish strong hopes that their strict suspension of all new plumbing work, and their active crusade against bad milk, will notably improve the city's sanitary condition in future by diminishing the cases of zymotic disease.

One case of typhus fever was discovered in the city during the week ending Sept. 24, but the premises upon which it was detected were so thoroughly fumigated and cleansed that no extension of the disease is apprehended. All the infectious diseases have diminished to a very encouraging extent during September. Only 46 cases of smallpox were reported as contrasted with 75 in August. Diphtheria diminished from 355 in August to 276 in September, while scarlatina and rubeola fell from 320 and 96 in August to 259 and 36 respectively in September. So excessive was the September heat that a number of sunstrokes occurred during the month.

The *American Gynecological Society* held its sixth annual meeting in New York, at the hall of the Academy of Medicine, on Sept. 21, 22, and 23. Owing to the absence of the President, Dr. Byford, of Chicago, the duties of presiding officer were discharged by the First Vice-President, Dr. Thaddeus A. Reamy, of Cincinnati. The address of welcome to the delegates was delivered by Dr. For-dyce Barker, President of the New York Academy of Medicine. Reception were held during the session of the Society by Drs. Isaac E. Taylor and T. A. Emmet. Lunch parties were given by Drs. Sims, Thomas, Barker, Byrne, and Skene. Papers were read by Drs. Thomas; A. H. Smith, of Philadelphia; Campbell, of Augusta, Ga.; Taylor, of N. Y.; Goodell, of Philadelphia; Lyman, of Boston;



Garrigues, of N. Y.; and others. The officers elected for the ensuing year are as follows: President, Dr. T. A. Emmet, of New York; Vice-Presidents, Dr. George H. Lyman, of Boston, and Dr. E. Noeggerath, of New York. Council, Drs. Geo. H. Bixby, of Boston; Jas. D. Trask, of Astoria, N. Y.; H. J. Garrigues, of New York; and G. J. Englemann, of St. Louis.

---

*Epidemic of Typhoid Fever.*—An endemic of typhoid fever which recently occurred in the Roman Catholic Orphan Asylum was believed to be due to the poor quality of the milk with which the institution was supplied. The analysis of the milk, made by the officials of the Health Board, showed it to have been skimmed and watered.

---

*Pink Eye.*—Numbers of horses in the large stables of various New York car companies have recently been attacked by influenza. It is called "pink eye" by the hostlers. Its symptoms are oedema of the legs, conjunctivitis, Schneideritis, tonsillitis, and bronchitis. Veterinary surgeons believe that the conjunctivitis may be relieved by steaming the animals' heads. The disease has thus far been of a mild type.

---

*The British Medical Association.*—The forty-ninth annual meeting of this Association commenced at Ryde on the 9th of August, and lasted four days. Professor Humphry, of Cambridge, President of the Association, was in the chair. Mr. Benjamin Barrow, of Ryde, was unanimously elected President for the ensuing year, and Mr. Humphry, in a few well-chosen words, handed the office over to his successor. Mr. Barrow delivered the customary presidential address.

Mr. Fowler, the General Secretary, read the annual report of the Council, which, among other things, stated that the roll of members now reached 9202, and that the next meeting would be held at Worcester, 1882, being the jubilee year of the Association, and Worcester the town where it was first started by Sir Charles Hastings fifty years previously. Dr. Strange was nominated for the presidency of that year.

Dr. Bristowe delivered the address in Medicine, Mr. Jonathan Hutchinson that in Surgery, and Dr. Sinclair Coghill, of Ventnor, that in Obstetrics.

At the annual dinner of the Association, Dr. J. S. Billings responded to the toast of "the Army of the United States."

---

*Owen County (Ky.) Medical Society.*—The physicians of Owen County, Ky., organized in September a County Medical Society, and elected Dr. J. W. Johnson, President, and Dr. R. I. Peck, Secretary.

---

*Vitiated Sea Breezes at Coney Island.*—The summer residents of Coney Island, New York, having bitterly complained of the odours emanating from the fat-rendering establishments on Barren Island, the Committee of the State Board of Health has visited the premises, and proposes to suppress the nuisance in question.

---

*King's County Lunatic Asylum.*—In his annual report upon the condition of the King's County Lunatic Asylum, in Flatbush, L. I., Dr. Shaw, the resident-physician, shows that, according to the statistics of the last two decades (from 1860 to 1880), the percentage of insane persons to the population of the county decreased from thirty-eight to twenty-nine per cent. Dr. Shaw has employed a system of non-restraint, whenever practicable, and reports excellent results from this method. During the year ending July 31, 1881, 428 patients were admitted

to the asylum, which now contains 868 inmates. Warden Murray, of the King's County Almshouse, reports that 3255 indigent persons were cared for during the year ending July 31, and that 673 still remain in the institution.

*Resignation.*—Dr. D. L. Schenck recently resigned the superintendency of the Flatbush Hospital, Brooklyn, N. Y., after occupying that position for seventeen years. Dr. H. O. Plimpton has been appointed temporary superintendent by the Commissioners of Charities and Correction.

*Oleomargarine Redivivus.*—A so-called "Oleomargarine Bill," in accordance with the requirements of which all oleomargarine exposed for sale was to be plainly marked, passed the New York Legislature, but was vetoed by Governor Cornell.

*The New York Night Medical Service* has now been in operation about a year. During that time five hundred calls have been made by its corps of physicians. It has been proposed to establish a day medical service which shall be conducted on a similar plan.

*Poisoning by Gaultheria (Wintergreen).*—Dr. M. L. FICHTNER, of Cranesville, Preston County, West Virginia, sends a report of the following cases of poisoning by oil of wintergreen: Rauham C. and Malinda C., æt. respectively 14 and 15 years, drank, through mistake, on the 4th of August, 1881, the former about 3j, and the latter about 3ij, of the oil of *Gaultheria procumbens*. Both were seized with vertigo, weakness, hot skin, frequent pulse, cold sweats, laboured respiration, and dulness of hearing. In the latter there was also inability to speak, with cramp in epigastrium and convulsions. Warm water and salt were then given as an emetic, and followed by olive oil. In the former case olive oil was continued in half-teaspoonful doses three to four times a day for about ten days, and the patient recovered without any serious inconvenience. In the latter case, bromide of potassium, olive oil, and the decoction of slippery elm bark were used, with cold applications to the head. The patient died in about ten hours after the drinking from congestion of the brain and convulsions. The breath had a pungent odor, and the gaultheria was detected in the perspiration. The patients were treated, at first, by Dr. Daniel Fichtner, afterwards Drs. A. S. and M. L. Fichtner were called.

*The Summer Home of the Children's Aid Society*, at Bath, Long Island, which was opened on June 18th, has already entertained 2800 poor children from the tenement houses. About 200 children are received, every week, at the home, and are returned to the city, after a week's sojourn at the institution, to be succeeded by an equal number of new-comers.

*Literary Notes.*—In the *Archives Générales de Médecine* for September, Prof. DUPLAY gives an elaborate "critical review" on the treatment of aneurism by the elastic bandage entirely based on the able paper of Dr. Lewis A. Stimson, of New York, which appeared in the *American Journal of the Medical Sciences* for April last. Dr. Duplay translates and adopts as his own large portions of Dr. Stimson's paper together with his conclusions, but unkindly misspells his name and that of Dr. R. F. Weir, of New York, almost beyond recognition.

The "Atlanta Medical and Surgical Journal" has changed its name to *Atlanta Medical Register*.

G. P. Putnam's Sons announce a "Treatise on the Science and Practice of Medicine," by Dr. A. B. Palmer, Professor of Practice of Medicine in the University of Michigan; and have just published a volume on "Eczema and its Management," based on an analysis of 2500 cases, by Dr. L. Duncan Bulkley, of New York.

—  
OBITUARY RECORD.—Died, at Buffalo, on the 28th of September, after a brief illness, aged 70 years, JAMES PLATT WHITE, M.D., Professor of Obstetrics in the University of Buffalo, and formerly Vice-President of the American Medical Association.

Dr. James P. White, a descendant of Peregrine White, one of the "Mayflower" colonists, was born in Austerlitz, Columbia Co., N. Y., March 14, 1811. He received his early education at the Middlebury Academy; his medical studies were commenced at the Fairfield Medical College, where he attended lectures during the years 1831-2-3, and at the Jefferson Medical College, Philadelphia, from which he graduated in 1834. He began professional practice in Buffalo immediately after graduation, and continued in active work almost until his death. Dr. White was one of the founders of the Medical Department of the University of Buffalo, where he occupied the Chair of Obstetrics and Gynecology, and was in 1850 the first in this country, it is believed, to teach midwifery clinically. He was a prominent member of the American Medical Association, its Vice-President in 1870, and one of the Vice-Presidents of the International Medical Congress held in Philadelphia, in 1876, and a member of various medical societies. His contributions to medical literature during the last thirty years were numerous and on various subjects, and were mainly published in the *Buffalo Medical and Surgical Journal*; the papers which, however, served the most to establish his reputation were on "Chronic Inversion of the Uterus," published in the *American Journal of the Medical Sciences* in 1858 and 1874. He also was the author of the chapter on "Pregnancy" in Beck's *Medical Jurisprudence*.

Dr. White was a man of strong individuality and wide influence in both public and private life; he was possessed of great executive ability and public spirit, and leaves behind him many warm friends. As a specialist he acquired an extensive and enviable reputation.

—  
CORRESPONDENCE.

1300 SPRUCE ST.,  
PHILADELPHIA, Oct. 7, 1881.

MR. EDITOR: I have been asked by several of my medical friends for some comments on the case of President Garfield, which was, during its progress, I think, most uncourtously if not unprofessionally appropriated through the newspapers, by some surgeons, as their own. If they had had anything of importance to communicate, I am sure the gentlemen in charge would have given them respectful attention, for access to the White House and to the offices of those gentlemen by mail was not cut off. The case was a public one to members of the profession as citizens only, and doctors not being in possession of all the facts to make intelligent criticism, should have remembered that they were in no position to share "the credit of success or the blame of failure." Now since the death and the official publication of the results of the autopsy, the case has become public, both to professional and laymen, and doubtless its diagnosis, prognosis, and treatment are fair subjects for comment.

From Dr. Frank H. Hamilton's "Military Surgery," New York, 1865, page 338, I make the following extract:—

"In a few cases a ball has been known to pass through the side of the body of one of the vertebra, leaving a round hole or a lateral furrow, *without coming in contact with the spinal marrow or the bloodvessels. It is not probable that we shall be able to diagnosticate such a case clearly during the life of a patient, and if we were able to do so, we do not see what benefit could be derived from any surgical operation.*

In case, however, one of the transverse processes has been broken and sent inwards, although it is not likely to have penetrated the cavity of the abdomen, it may yet give rise to serious results by the *formation of an abscess in the bellies of the psoas muscles, which abscess may eventually make its way along between the fibres towards the groin, or may empty itself into the loose areolar tissue outside of the peritoneum.*"

The italics are mine. Could there possibly have been a more wonderful foreshadowing of the President's case? The fracture of the transverse process is replaced in the case in hand by the fracture of the body, which produced the abscess or channel along the psoas in precisely the same way as described by Dr. Hamilton. The description is perfect. The sinus or channel which was, in the words of the official report, supposed during life to be the track of the bullet, was practically an abscess and sinus caused by the disintegration of a vertebral body through gunshot, precisely as a chronic lumbar and psoas abscess is caused by the chronic disintegration of a vertebral body through disease. How truly Dr. Hamilton sets forth the difficulties of diagnosis, and how absolutely true he is as to surgical interference by operation, having for its object the getting of the missile, which, as the autopsy shows, was lying harmless, compared to the injuries it had inflicted. The diagnosis then was hard; but does it not seem, in the light of our present knowledge, that the deep significance of the nervous symptoms of the President immediately after the shooting, was lost sight of too early in the case? This, if so, was probably the result of the exciting surroundings, and of the fact that the doctors were more occupied in practical efforts to relieve these symptoms than in speculations as to their cause. They were attributed to injuries or impressions upon nerves of the lumbar plexus, instead of to direct shock to the cord itself. This view seemed to be confirmed by the fact that they promptly subsided under rest and other treatment. That temporary spinal shock does sometimes occur at once after an accident to the column, I will show further on by an actual recent case.

But those pains in the legs, those "tigers' claws," affected *both* sides; what would account for them so well as sudden injury of some kind to the spine? From their bilateral suddenness it is more reasonable to attribute them to direct action upon the centre, than to reflex transfer from the right to the left. The right side I assume exhibited them to a greater degree than the left, but the full force of the impact was received upon the right and was weakened upon the left. If I am not right and the pains were equal on both sides, then there is still stronger reason to refer the phenomena to a central origin.

I have read also, that at first there was hyperæsthesia of the genitals. If so, this, together with the "tigers' claws," pointed strongly to the spine. Some surgeons are sceptical about spinal shock. This summer, in July, Dr. Agnew assisted me in a case that confirms its occurrence.

A boy was shot in the back to the right of the third or fourth dorsal vertebra. He at once had characteristic symptoms in the legs of being

wounded in the spine. These soon disappeared. The course of the bullet was not readily made out at first, but the diagnosis was that the column was wounded without injury to the cord, as the symptoms were not continuous. After suppuration set in, the course of the ball was readily traced with a probe, and broken fragments of the vertebral processes were felt. The ball was not felt with a Nélaton or other probe. The patient was etherized. I made a large and deep incision through the muscles in the line of the wound, and removed some clothing and fragments of bone and finally a large bullet, which was lying immediately against the bony bridge of a vertebra. The boy recovered. Whether he will suffer from caries in the future remains to be seen. This boy certainly had spinal shock, without injury to the cord, for there was no remnant of paralysis when he was discharged from the hospital.<sup>1</sup>

But this case is very different from the President's in this, that it was amenable to operation, and we knew about where the ball was. Also here, the ball would have produced great future mischief. In the President's case it did its damage and then hid. If such difficulty occurred in the hunt for it as is stated at the autopsy, imagine the horror of a like attempt on a live man. How long would he have lived? He had better have been killed at a blow.

A surgeon who has *lived* in an accident hospital and receives cases day and night, immediately after injury, gets into the habit of making "snap" diagnoses. It is really not the presumption of ignorance, but it is the quick application of knowledge gained by experience. In nineteen cases out of twenty the "snap" is right; often he who deliberates is lost. Now excuse the egotism, for I have abundant proof of what I say, and I say it for the credit of diagnosis, "snap" though it be. At once, upon reading the accounts of the leg pains and the "tigers' claws," I said to a medical friend, and I have no doubt that many other hospital men said the same thing, "the President is shot in the spinal column." But after the diagnosis was given to the public stating that the direction of the ball was downward and forward, through the liver, and into the anterior wall of the abdomen, I came to the conclusion that in this case the "snap" had failed.

The question of the duty of the consulting surgeons when they first went to Washington has been freely discussed. Should they have reopened and re-examined the wound with probes and fingers? I am assuming that they did not do so, and they were right. A hospital chief, if he has confidence in his residents or assistants, and knows them to be experienced and ready, takes what they say about serious cases which will not tolerate officious disturbance. He recognizes the "snap" if the other symptoms confirm it. For example, the chief visits the hospital on his regular rounds. The resident says, "Doctor, here's a man who was brought in last night shot in the abdomen. He was greatly shocked; I examined him and passed my finger directly into the cavity; I removed some pieces of clothing; I can feel the bullet nowhere under the skin about the whole body; I searched carefully; I have dressed the wound and given some opium; he is now quiet and free from pain." The surgeon looks at the patient, sees that his general symptoms confirm the diagnosis, and that his wounds are carefully dressed. He probably re-examines the body

<sup>1</sup> Whilst reading proof of this article, October 18th, this boy walked into my office, having been kindly sent to me, by Dr. W. C. Cox, of the Out-Department of the Hospital. He was perfectly well in every respect.



externally, but if he is a wise man he says practically "hands off, we will await results and act accordingly."

Many a life has been saved by this course; many a one has been lost by deviating from it. I question if one has ever been saved, in the kind of case now under notice, by deviating from it. Now if a surgeon can so treat those who are *under* him, how much more is he bound (unless he sees something positively and absolutely wrong), to receive the statements of his peers, especially when outward appearances seem to confirm them. I question, moreover, whether any more light whatever would have been thrown on the President's case by further exploration at the time when the consulting surgeons made their first visit? By that time, the spinal symptoms had disappeared, or were very much alleviated. Inflammation through swelling of the denser tissues, muscular and fibrous, of the true track of the ball had doubtless closed it. The finger or probes would have passed easily where other fingers and probes had already been, and there would have been almost certainly a positive confirmation of the results of the first examination. When I study the autopsy more closely, I think that this would almost to a certainty have been the case. If I understand the description and the drawing, the ball entered at a place which, if it had had consciousness and an intent to deceive, could not have been better selected in the whole body. It entered "three and a half inches to the right of the vertebral spines," therefore its course began to the right of the psoas mass. Instead of going forward, it traversed the circle of the body transversely, after fracturing the eleventh rib, and went directly on in the direction of a chord of an arc of that circle. It entered the post-peritoneal space and thus penetrated the posterior wall of the abdomen, without opening the peritoneum. It then continued on into the muscular mass, separating the longitudinal fibres, which may have closed behind it. The finger was passed into the wound and also probes. These went readily on in the space mentioned, and the true course was missed. I have no doubt the depending liver, or it may be the kidney, was felt. The wound was one to deceive the very elect. The influence of the inner loin muscles in concealing injuries and diseases of the lumbar vertebra is well recognized. The post-mortem report speaks of the dilated track of the ball, but this dilatation was owing to subsequent changes. I am ignorant of any other explanation of the position taken by fingers and probes, than that I have given.

It is a mistake to suppose the spine escaped attention in the case. Dr. Agnew assures me, that when he operated for the relief of the lumbar abscess, he carefully searched the transverse processes and all of the spine he could feel, and felt nothing wrong. The injury to it was beyond his fingers, and a wise caution here forbade the use of instruments. How truly Dr. Hamilton's remarks are illustrated by this experience. Moreover, by the incisions Dr. Agnew made, which he tells me were carefully planned for drainage, the whole line of the true wound, as well as the mistaken sinus, had full outlet.

The abscess which was found after death, bounded by the liver, the transverse colon, and the transverse meso-colon, for anatomical reasons, was shut off from communication with the wound, and of course was not drained. In the words of the official report, "no communication could be detected between it and the wound." No method of diagnosis occurs to me by which its existence could have been determined during life, unless it had got far enough to cause posterior or anterior bulging.

It was an abscess, arising either from peritoneal inflammation through contiguity or it was a septic deposit.

The case really was one to diagnose more by the application of anatomy and physiological and pathological knowledge, than by direct methods. In the light of the autopsy, everything is explained. By the theory of the wound during life, almost nothing.

There were the pains in the limbs and the "tigers' claws." There was most profound depression and a pulse and temperature record which, after the liver theory was abandoned, the accepted wound, at most a severe flesh one, was insufficient to account for. There was the origin of the thoracic duct with its receptaculum chyli right in the line of the wound; hence, the rapid emaciation, and the other nutritive disabilities, further explained by disturbance of the sympathetic trunks and ganglia. There were the radicles of the vena azygos, bathed in the corruption of rotting vertebra, and hence the septicaemia, and there was the abscess and sinus, running toward the groin, a result of the rotting vertebra and not of the ball. What good could it have done to have cut for the bullet after its first mischief? It was then but a small factor in the troubles.

As to the liver, there are recoveries from wounds of it, but they are rare. Between twenty and thirty of such recoveries are reported I think in the surgery of the war. How many even of these were mistakes as to diagnosis? During my last term at the hospital, an old soldier, very peculiarly and deeply jaundiced, fractured his thigh. He told us that the jaundice was an old affair, that he had been shot through the liver in the war, and showed the external scar. He became very sick and died. The liver was most carefully examined, but there was no mark of a wound about it, nor in it. Serious wounds of the liver generally kill promptly by hemorrhage. Hundreds must be killed in this way in battle. On the other hand, recovery may take place and a bullet (strange as it may seem) may become completely encysted in the organ. Dr. Owens, now resident in the Pennsylvania Hospital, has a bullet with the encysting capsule around it which he took from the liver of an old fellow who died in the almshouse from some other cause. It must have laid there for many years.

As to the treatment of the President's case, it was in the light of the autopsy most fortunate. A very valuable life was prolonged beyond all reasonable hope. From a letter I had from Dr. Agnew, dated the 23d of August, 1881, which was confidential then but open now, I know how fully he appreciated the profound gravity of the case, and took an almost hopeless view of it, but still he with his colleagues fought it out nobly. When we come to think of it, it may be a mercy that an exact diagnosis was not made. The temptation to do something more than was done, if it had been made, would have been very great. Outside and inconsiderate pressure would have been clamorous. Whether it would have moved the steady heads in charge, I do not know; but if it had, I am confident the President would have been ready for his grave on the day of the operation. Who can picture the political results that might have followed? Now the perturbing elements have been calmed, and all is peace.

Much has been said about antiseptic treatment in the case. It was practised, I believe, to a very great extent. Its *practice* is grand, except the spray; I heartily agree with the German surgeon who said "*fort mit dem spray.*" Could there be a better commentary on the *unproved theory* of antiseptic surgery than the President's case? Against its theory, or at

least the effect of it on many minds, viz., that all sources of contamination producing septic poisoning come from without, I earnestly protest. I have too much respect for Mr. Lister to think that he believes what the extremists among his disciples teach.

The influence of such teaching on the rising generation of physicians and surgeons is bad, in this, that it leads to narrow views and interferes with clear diagnosis. It leads outwardly too much, for contamination comes from within, I believe, more frequently than from without. Was not the decaying vertebra in the President's case enough to account for the septicæmia?

To get all the rats out and then stop the hole with poison and stuffing is a good thing, but to poison and plug the hole and leave the rats in, is a very bad thing. They only undermine and make other holes. A narrow antiseptist looks around the room instead of at the patient, whereas, he should look at both and give due weight to all septic possibilities.

Discussion is now going on as to whether the President's wound was mortal or not. What is mortal? Essentially mortal wounds are extremely limited. A sweep through a certain portion of the medulla oblongata, which is itself a small organ, or a wound opening the aorta, may be instanced as examples. Haller's tripod of life was the heart, the lungs, and the brain. One leg of this tripod gone, the rest falls. The leg must be thoroughly undermined, or else injured in a particular part. Many people get well of wounds of the brain and lungs. Recoveries occur from injuries to the muscular substance of the heart without opening its cavities. Foreign bodies lie imbedded in that substance for years. I saw a large needle taken out of the heart of a negro in the dissecting-room, which must have been there for a very long time, and had nothing to do with his death. Now, setting aside everything else, what was the immediate cause of the President's death? It was from what, in extent of tissue involved, was the smallest serious feature of his injuries. It was the rupture of a traumatic aneurism of the splenic artery! I know of no method in science that would surely locate such an aneurism. If it were located, I know of no measure that would stop its progress. That progress is steadily on to rupture. Anatomy surrounds it with no special sustaining structures. That rupture was essentially fatal, and so with other injuries that might have killed, this one positively did kill.

Such an improbable course as the following was possible in the President's case. The development of the aneurism might have been slower. The bullet was already encysted. The injured vertebra might have cast off its decaying debris, and it, together with the rest of the wound, might have healed. The President might have been pronounced well, and even have resumed his duties. Some day, suddenly, an agonizing cry of pain would have escaped him, and he would have fallen and died as though he were shot a second time! No one would have known the cause until an autopsy revealed the ruptured aneurism. It is but a piece of special pleading to say the wound was not mortal.

WILLIAM HUNT,

Senior Surgeon to the Pennsylvania Hospital.

---

*To Readers and Correspondents.—The Editor will be happy to receive early intelligence of local events of general medical interest, or which it is desirable to bring to the notice of the profession. Local papers containing reports or news items should be marked.*

# CONTENTS OF NUMBER 468.

DECEMBER, 1881.

## CLINICS.

### CLINICAL LECTURE.

	PAGE
On some Catarrhal and Muscular Disorders of the Stomach. A Clinical Lecture. By MORRIS LONGSTRETH, M.D., one of the Attending Physicians to the Pennsylvania Hospital. . . . .	707
Clinical Lecture on the Use of Sponge Pressure as a Surgical Dressing. Delivered at the Royal Infirmary, Manchester. By JAMES HARDIE, Assistant-Surgeon to the Infirmary . . . . .	715

### HOSPITAL NOTES.

Aneurism of the Subclavian and Axillary Artery treated by Galvano-puncture. By Mr. Ransford . . . . .	720
---	-----

## MONTHLY ABSTRACT.

### ANATOMY AND PHYSIOLOGY.

PAGE	PAGE
Influence of the Nerves of the Tympanic Cavity on the Vascularity . . . . .	and Secretion of its Mucous Membrane. By E. Berthold . . . . . 723
	Electrical Tetanus. By M. Richet . 724

### MATERIA MEDICA AND THERAPEUTICS.

Action of Berberin. By Dr. A. Curci . 725	Papaya and Papain. By M. Bouchut . . . . . 728
Influence of Anæsthetics on the Heart, and on the Antagonism of Poisons. By Dr. Ringer . . . . . 726	Hypodermic injection of Citrate of Iron. By Dr. Ciaramelli . . . . . 729
Action and Use of Citrate of Caffein as a Diuretic. By Dr. David Brakenridge . . . . . 727	Hypodermic Injection of Water in the Treatment of Pain. By Dr. Ponte . . . . . 729

### MEDICINE.

On a Peculiar Form of Disease arising from Milk Contamination. By Mr. R. Beveridge, M.B. . . . . 730	Paralysis. By M. D. Macleod, M.B. Ed. . . . . 734
Epithelial Necrosis and Diabetic Coma. By Prof. Ebstein . . . . . 731	Spinal Lesions from Compressed Air. By M. Paul Bert . . . . . 735
Treatment of Neuralgia and Rheumatism by Electricity and Iodoform. By Dr. Mosso . . . . . 733	Nerve-Stretching in Diseases of the Spinal Cord. By Dr. Carl Langenbuch . . . . . 735
Cerebral Symptoms in Dyspepsia. By M. Leven . . . . . 733	Ergot in the Paralysis of Lead Poisoning. By Dr. J. A. Stites . . . . . 736
A Case of Destruction of the Corpus Striatum without Symptoms. By Honegger . . . . . 733	Treatment of Eriopyema. By Dr. W. Wagner . . . . . 737
Left-sided Convulsions followed by	Incision of the Pericardium. By Prof. S. Rosenstein . . . . . 740
	Severe Chronic Gastric Catarrh

PAGE	PAGE
treated by Washing out the Stomach. By Prof. Riva . . . . .	741
Hydatid Tumour of the Liver in a Young Child . . . . .	741
Filaria Sanguinis Hominis. By Dr. Stephen Mackenzie . . . . .	742
Psoriasis from Borax. By Dr. W. R. Gowers . . . . .	744
Gangrenous Eruption in Connection with Chicken-pox and Vaccination. By Mr. Jonathan Hutchinson . . . . .	744

## SURGERY.

Operations for Chronic Disease in Phthisical Patients. By Mr. Bryant . . . . .	746
New Methods of Treating Erectile Tumours. By M. Constantine Paul . . . . .	748
Visceral, especially Renal, Syphilis. By M. Barthélemy . . . . .	748
Aspiration of Suppurating Buboec. By M. Le Pileur . . . . .	749
Scrotal Calculus. By Lippoman . . . . .	749
A Case of Popliteal Aneurism Cured by Means of Esmarch's Bandage and Digital Compression. By E. Z. Derr, M.D. . . . .	750
Rupture of the Plantaris Muscle. By Dr. A. B. Judson . . . . .	750
Experimental Researches on the Forcible Straining of Genu Valgum. By M. Menard . . . . .	751
A Form of Gonorrhœal Arthritis. By MM. Duplay and Brun . . . . .	751
Treatment of Floating Bodies in the Knee. By Dr. G. Gaujot . . . . .	752
Rare Accidents. By Mr. Charles Melvhor Goyder . . . . .	753

## MIDWIFERY AND GYNÆCOLOGY.

The Mobility of the Pelvic Articulations. By Dr. Korsch . . . . .	754
A Peculiar Condition of the Cervix Uteri which is found in Certain Cases of Dystocia. By Dr. Alfred Hosmer . . . . .	755
Exfoliative Vaginitis and Membranous Dysmenorrhœa. By Dr. Cohnstein . . . . .	755
The Origin of Tubo-Ovarian Cysts. By Dr. Henri Burnier . . . . .	756
Incomplete Removal of Ovarian Cysts. By M. Terrier . . . . .	757

## MEDICAL JURISPRUDENCE AND TOXICOLOGY.

Detection of Arsenic in Wall Papers. By Dr. William B. Hills . . . . .	757
Poisoning by Carbolic Acid . . . . .	758
Case of Poisoning by Atropia. By Reiml . . . . .	758
An Accident with Hydrofluoric Acid. By Mr. Robbins . . . . .	759

## MEDICAL NEWS.

To Subscribers . . . . .	761
Ovariectomy under "Modified Listerism" . . . . .	762
Early Experiments in Bovine Inoculation . . . . .	762
Hospitals for Contagious Diseases . . . . .	763
Rules of Authorship . . . . .	763
The Laryngological Congress . . . . .	763
Health of New York . . . . .	764
Listerism at Montpellier . . . . .	764
A Solvent for Quinia Sulphate . . . . .	764
The New Manhattan Eye and Ear Hospital . . . . .	764
A Case of Recovery after prolonged Immersion . . . . .	765
Deliquescent Salts in Street Watering . . . . .	765
The Bressa Prize . . . . .	765
Obituary Record :	
William Furness Jenks, M.D. . . . .	765
Alfred McClintock, M.D., LL.D., F.R.C.S.I. . . . .	766
Prof. Jean Baptista Bouillaud . . . . .	766
David Foulis, M.D. . . . .	766